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Journal

A

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Mrs. Agnes Ibbetson; Thomas Forster, Esq.; J. A. De Luc, Esq. F. R. S.; Mr. Thomas Shute; Thomas Knight, Esq.; Dr. Baird, Physician-General to the Navy; G. Cumberland, Esq.; Mr. P. Barlow, of the Royal Military Academy, Woolwich; John Wingfield, Esq.; Mr. John Cuthbertson; Marshall Hall, Esq. F. R. M. S. E.; Mr. F. Kerby; Mr Merrick, jun.; William Moore, Esq. of the Royal Military Academy, Woolwich; Mr. T. Noot; Thomas Thomson, M. D. Lecturer on Chemistry at Edinburgh, &c.

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The Engravings consist of 1. Figures illustrating the Growth of Seeds, in those of the Peach, Chestnut, Grass, and Palms, delineated after Nature in various Periods of their Evolution, by Mrs. A. Ibbetson; 2. Mr. De Luc's Electric Column, and Aerial Electroscope; 3. Mr. Shute's Scarificator on a new Principle; 4. Diagrams to illustrate the Theory of Capillary Attraction, by Thomas Knight, Esq. 5. Figures to illustrate the Structure and Classification of Seeds, delineated from Nature, by Mrs. A. Ibbetson; 6. A Machine for raising large Stones out of the Earth, by Mr. Richardson; 7. Apparatus for Experiments on the Sonorous Properties of Gasses, by Mr. F. Kerby and Mr. Merrick, jun.; 8. Crystals of Apophyllite, or Fisheye Stone, by Mr. Haüy; 9. A new Hygrometer for Gasses, by Mr. Guyton-Morveau; 10 Delineations of Mr. Congreve's Military Rockets; 11. The Cancer-Fulgens, discovered by the Right Hon. Sir Joseph Banks, of the natural Size; 12. The same Animal magnified; 13. The Medusa Pellucens, also found by Sir Joseph Banks, represented of the natural Magnitude.

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A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

SEPTEMBER, 1810.

ARTICLE I.

On the Structure and Growth of Seeds. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

HAVING been requested by a gentleman, highly esteemed in the botanical world for the knowledge he has displayed in that science, to review the formation of seeds in general; to give a clear and concise picture of the growth of the embryo plant, from the first of its appearance in the seed vessel, to its shooting a perfect plant from the earth; to endeavour to prove the mistakes the variety of appellations have caused, as well as the misconceptions its extreme minuteness naturally occasions; and to show also in what order the several parts appear, as physiologists have differed much in this respect: honoured by such a request, I shall venture to begin my task, trusting in the great magnifying powers of the various excellent instruments we now possess, and apologizing for venturing to contradict authors so much superior to me in science, as in this matter the

The author's inducement to write on the subject.

eyes are the principal judges, and a long habit of viewing diminutive objects will detect truth, sooner than much knowledge. As I have before written on this subject, it will not be wholly in my power to avoid repetitions; but they shall be as few as possible.

Cause of the mistakes of physiologists.

It appears to me, that the manner in which seeds are generally taken for dissection is the cause of most of the mistakes disseminated. Most physiologists begin by dissecting full grown seeds; and, following their first impressions, applying the names, and appropriating the descriptions, to such parts of the seeds (in that state of growth) as appears best to suit them. But as there is not in nature any thing more different, than a very *young* seed, and an *old* one; it is ten to one that the names are misapplied, as the vessels of most consequence in the earliest stages are wholly lost in an old seed, while another set of vessels usurp (*almost*) their places: by which means the nourishing vessels, at the termination of their career, are often mistaken for the impregnating duct; while the vessel of life (before it reaches the heart) is taken for the *radicle*. So easily are these mistakes to be traced in most works of this kind, that I could give innumerable examples to prove my assertion, were it not better to show the origin of the mistake, and endeavour to rectify it; proving plainly, that to begin by dissecting full grown seeds is truly commencing at the wrong end.

Young seeds & old ones differ essentially.

Numerous instances of mistakes from this source might be adduced.

The seed should be examined in its earliest stage, and pursued through its whole progress.

To gain a perfect knowledge of the seed, it is necessary to detect its first appearance in the seed vessel; in this situation become perfectly acquainted with the names and uses of each part; and thence trace them upwards in their daily improvement, till, removed from the seed, they are fixed in the earth. To distinguish the vessels designed for the seed only from those intended to complete the *fruit*, is of the *first* consequence: for, as the seed is the principal *object* of Nature's care, it is the first perfected in its *vessels*; but, after they are all ready for the completion of the embryo, it takes a long time to prepare the young plant for its first appearance; and this time is employed by Nature in the completion of the fruit. This has, in most instances, two or three different vessels, not in any manner useful to the seed, found at this time invigorating the fruit; and as these

these mix much with the vessels of the seed, if the latter are not previously well known, they are but too apt to produce a very great confusion.

When the bud of the flower first appears, it is a notice Nature gives, that the seed has received *life*. It is then time to delineate the seed vessel, when we can best distinguish all the vessels of consequence, and perceive from which part they come, and for what office they are ordained. They are of three sorts. First, the impregnating, or vessel of life. This is always found in the middle of the seed vessel, proceeds directly from between the wood and pith in the stem of the tree, and becomes only the impregnating duct, when after entering the pistil it mounts to the stigma, and there receives the flower of the stamen, to mix with its own juices. Mirbel says, "The vessels of the style unite in the placenta, with those of the peduncle, and compose with them the umbilical cord." This is certainly a mistake: for the style is merely a *sheath* for the vessel of life, which only becomes the *impregnating* or *umbilical* vessel, when it returns from the stigma, conveying to the corculum of each seed the *joint juices*. This string I have often taken out at its length, after boiling away the pulpy part, and I could never perceive it was joined to any other.

Commencement of life in the seed.

Three sorts of vessels in it.

Vessel of life.

Mistake of Mirbel.

The second is a set of vessels called the nourishing vessels. These have not been long discovered, and I believe I was the first botanist that announced them. They run from the bark and inner bark, and bring not only support to the embryo, but the resins, oil, &c. for the formation of the young leaves. They enter the seed in bundles, and are by degrees disseminated all over it.

Nourishing vessels.

The third vessel is a single one, and contains the hidrogen. It proceeds from the exterior of the plant, and enters at the back of the corculum or heart of the seed; but not till the formation is almost complete. It may indeed be said to give it the last finishing touch.

Vessel containing hidrogen.

There are also three periods in the life of the embryo plant, concluding with its no longer meriting this name, which, for want of being properly discriminated, have caused the greatest confusion among botanists. The first is from the time the seed appears in its seed vessel to its impregna-

Three periods in the life of the embryo.

tion: the second from its impregnation to its readiness for planting: and the third from its being placed in the earth, till it rises thence as a growing plant. As I have taken many hundred seeds in this progressive manner, I could show their almost daily increase in regular drawings: but this would take far too much room. I shall however give in description as much as possible, adding a drawing of three or four of the intermediate times, to make it perfectly intelligible. For this I shall fix on the peach seed, the manner of growing of which is most generally applicable to all; the horse chestnut; and one of the grasses; to show any curious irregularity in the stated laws usually admitted. So very much do the interior of seeds resemble in their general features, that these three may serve as an epitome of all; for as in every seed there are eight parts common to all, it may well be supposed in so diminutive an object how great must be the general resemblance.

Eight different parts in every seed.

The eight parts are as follows: 1st, the line of life, or impregnating duct; 2d, the nourishing vessels; 3d, the corculum; 4th, the inner skin or pocket; 5th, the cuticle, or outward skin of the kernel; 6th, the stem of the embryo; 7th, the radicle; 8th, the cotyledons. I shall now describe the different stages of seeds, and confine myself to those circumstances, that are to be found almost *universally applicable*; and should I be thought prolix (which it is hardly possible to avoid on such a subject) I hope I shall be pardoned, if I am perfectly intelligible. I shall begin with a seed vessel of the chestnut cut both ways, as it is absolutely necessary to show the manner in which the different vessels may be traced in the seed vessels. Plate I, fig. 1, shows the seed vessel cut horizontally. It marks the distance at which different vessels enter. It is very curious to observe how it varies in place, till confined within the groove of the future stalk. Fig. 2 is the seed vessel cut lengthwise, to show how important a figure the vessel of life makes, when the seed is so young, and before impregnation: for at a later date it is hardly to be discerned, the pabulum of the seed having so increased in thickness, as to cover and inclose it. Though in both these there appear the rudiments of many seeds, yet it rarely happens, that there are more than one

Seed vessel of the horse chestnut.

or

or two perfected; while the accumulation of juices prepared, and even formed into albumen, for the others, appears again to be dissolved into liquid nourishment for the favoured embryo. A beautiful provision of Nature: for, if those that did not arrive at maturity were to decay, so very much is the rot in vegetables liable to spread, few probably would perfect their seeds; whereas of the number that grow most of them serve as a reservoir of juices for the use of the most healthy of those the seed vessel contains. It is not difficult by long habit (when there are few) to discover their number by the protuberance of the outward cuticle: yet after this they disappear, and leave little or no vestige *but* a skin, *if that.* This is mostly however to be found in the seed vessels of trees, the glandiferes, nuciferes, &c., where the juices are easily communicated from one to the other.

Many seeds never perfected, but prepare juices for the others.

In all seeds there never is but one embryo to be found in one kernel. The vessels in both halves should be traced with care to their origin, the impregnating vessel to its interior, the nourishing vessels to the exterior of the seed vessel, which suggests (what is afterward proved) that the latter receives much addition to its juices from the *dews* and *vapours* around.

The seeds receive part of their nutriment from without.

I must now mention what no one has before noticed, but which is, I am persuaded, in part, the cause of the mistakes respecting the radicle, and the time of its appearing to grow in the seed. When the seed is taken in the seed vessel it shows a string (see *a*, *a*, *a*, *a*, figs. 1 and 2) running often, before it reaches the corculum of the seed, nearly round, and at a distance from the outward cover. Now this string, I am persuaded, has been taken for the radicle; or how could any botanist describe the radicle as preceding the cotyledons, when often *two months intervene* between them, the latter always shooting first? I will be bold to say, that the radicle of no seed ever did grow, till the *last epoch*, or its return into the earth. This string, which is the impregnating vessel, cannot enter into the seed, except at the corculum: it must therefore *stretch in length*, till it reaches this part, as the seed is not yet fixed, but swims and moves in a clear fluid. It may be observed in

Part mistaken for the radicle.

in the plate, that scarcely *one* of the *seeds* given is turned the *same way*: but when the seeds leave the capsule, the vessels are confined within the stalk; besides, the business of the impregnating vessel has then been long over, and the string is either lost in the seed vessel, or melted away into nourishment for the rest; for it no longer appears: though I have my doubts, whether it is not the string found afterward passing over the kernel.

Periods in the growth of the seed.

I now turn to the different stages of the growth of the seed. Figs. 3, 4, and 5, will display the kernel in the earliest stage of its first period. The pocket, *d*, is then filled with a transparent jelly. The corculum, *c*, is perfectly *empty*, being only distended by the parts adjoining. The impregnating vessel, *a*, joins the corculum to the pocket, which is still seen at some distance. Beside these vessels of *consequence*, there are generally found in the first stage some bringing air, others juices, to the new plant. I never before, except in water plants, discovered a real *air* vessel *in a vegetable;* but in the seeds they are certainly found, and it is almost impossible to conceive what a quantity these *little vessels yield*, and how continually the change of vessels is seen; each day brings a new assortment.

Commencement of the 1st period.

Air vessels in seeds.

End of the 1st period.

In the latter part of this period the pocket has greatly increased, and of course shortened the distance between the two bags; the nourishing vessels have entered the outward cover of the seed; and, instead of the air vessels before seen, a number of vessels running from the green part appear to assist in increasing the pocket. And here ends the first period.

Beginning of the 2d period.

The second begins with the impregnation of the embryo; and the first sign of it is, that the corculum is immediately filled with a glutinous liquor. Mirbel says, "Some time before and after impregnation, no change takes place in the interior of the plant." I have so long studied this part, that I must contradict him. I never knew the second noon pass, if the plant was affected, without making the impregnation visible; though I have certainly seen the stamen flower, without any consequence taking place with respect to the seed; but then the seeds *never germinate*, and if there was no visible effect, I cannot conceive how it could

Mistake of Mirbel.

be

be known, that impregnation *had taken place*. On the contrary, the alertness of Nature is such, that often in 48 hours the corculum is filled, the impregnating line runs through the heart, and the increase of the nourishing vessels is visible. The line stops when the cotyledons shoot, but they do not yet appear.

In the second stage of this period the spreading of the nourishing vessels is astonishing; the pocket and corculum join; and the cotyledons begin to grow. All this time no radicle appears, though the cotyledons have almost completed their form. It is in this state of advancement, that the seed declares whether it is a *seed leaf*, or *common seed*: for these two forms divide almost the whole assemblage of seeds. 2d stage of the 2d period. Two divisions of seeds.

The conclusion of the second period shows the cotyledons in their natural form: that is, either with two little silvery thin leaves, perfectly white; or with two thick *yellow leaves*, which afterward, rising with the plant, turn green, and are seen above the earth. I have never seen more than two, except in the fir tribe and some of the grasses; and in the cress and mustard seed, the former of which has six cotyledons, and the latter four. This perhaps accounts for their springing so quickly out of the earth. With respect to the monocotyledons and dicotyledons I shall say a few words toward the conclusion of my letter, as I am perfectly convinced with Wildenouw, that the division is erroneous. This is the time for proving, that no radicle is yet to be seen. Where is the radicle in these seeds ready for planting, and prepared for it? see figs. 15—21. Last stage of the 2d period. Cotyledons.

When the radicle begins to shoot (and its work is soon done), the primordial leaves also show themselves between the cotyledons, see figs. 22—25: and it is in this stage, that the holders begin to grow, where there are such parts in the seeds; for few plants, in comparison of the innumerable species, *have them*. In some of the diadelphian tribe indeed they grow sooner, particularly in the beans. At the termination of the second period too is seen the use of that part in the seed, which is formed with peculiar strength, and not only marked where the vessels enter, but which a double cuticle covers; either in a *circular* spot, as in the *chestnut*; Radicle.

chestnut; or in a long *cylindrical slip*, as in the pea. So violent is the force used against it, that, if it was not by various means greatly *strengthened*, the embryo would burst from the seed, long before its time, probably destroy itself by a premature birth, and tear the vessels in a manner highly inimical to their future *usefulness*. How beautiful the provisions of Nature! What care, what attention, to each minute circumstance!

First stage of the 3d period.

In the first stage of the third period the radicle, grown too large for its prison, bursts the seed, and comes forth at the hilum or opening: which, defended by the double cuticle, will only admit of a certain aperture in some seeds, while in others it divides the lobes.

I shall not enter into a thorough detail of the manner, in which the embryo rises and turns to leave the seed, as I have given an exact description of it in my former letter; entering very minutely into every particular of that phenomenon; but proceed to the last stage, which concludes the whole history of the embryo, fixing it in the earth, and raising its head on high. The seed lobes however continue fastened to the fresh plant, lest any accident should happen to the root: for should this be the case, the nourishing vessels, still remaining on the holders, would reassume their office, regain their former fulness, and with the help of the albumen (of which the seeds still retain a certain quantity) nourish the young plant, till the radicle had recovered strength sufficient again to supply its place. I have so often proved this fact by severing the new root to try its effect on the embryo, that I am well assured of its reality: as it never failed to produce these consequences.

Last stage of the 3d period.

Accidents to the root provided against.

I could have diversified this account, and perhaps made it more amusing, instead of a dry detail of facts: but I write merely to show the *truth*, and I wished particularly to confine this account to what happens to seeds *in general*, rather than to the seeds of any particular plant; that it might in some measure clear up the errours I so much lament: there is nothing more therefore to show, than that, as the radicle (which is always the *tap root*) touches the earth, the nourishing vessels decay; and the primordial leaves raise themselves with the stalk in a perpendicular posture. Seeds differ at this

The radicle always the tap root.

this time in one respect. In seed leaves they raise themselves, and continue to grow with the stalk; though they show by their outward form, that they have differed in their first manner of growing; while other seeds, having the silvery and delicate cotyledons, leave them in the earth, where they decay with the lobes. I cannot agree with Mirbel, that they afterward nourish the plant; in a very short time they (as well as the remnant of the seed) break off, and are lost in the earth; or serve as a nest for some of the numerous insects, that equally receive their nourishment from the bounty of the Almighty. Difference in seeds at this time.

I shall now show the conclusion of the chestnut and grass, the former of which differs in a curious manner. It possesses, like the peach and every other seed, the parts already mentioned, except the cotyledons, which it is wholly without. It is impossible to have watched it more narrowly, and to have magnified it to a greater degree; but it has certainly no cotyledons; and the reason why it has none is very plain, and shows distinctly the use of the cotyledons. It has such a length of stalk to the leaf, that the seminal leaves (without greatly exceeding their usual size) could not cover it. The primordial leaves therefore, with their stalk, shoot from the place the cotyledons usually show themselves at; and the stalk of course comes from the same. See the heart of the horse chestnut, fig. 15. This very plainly shows, that the cotyledons are of no other use to the embryo, than screening the primordial leaves from the light and air at their first formation. The esculus differs in no other manner from the peach in its seed; the holders are longer, but they in reality are no more connected with the interior, than the fruit; which I have purposely avoided mentioning, not to confound it with what nature esteems of so much more consequence. Seed of the chestnut. Without cotyledons. Reason of this. Use of the cotyledons.

As to the grass, it will at first sight appear to possess a part different from other seeds. After strict *examination however*, this is found not to belong to the seed, but to the valves of the grass, and to be the excrescence on which the stamens grow: and as to the small head on which the cotyledons rest, it is certainly a part of the heart, since all the vessels pass through it; and literally is that part, which is first Grass.

first *formed, in the grass seeds.* I shall say something more of this order of plants, when I come to explain the formation of the palms.

Many mistakes in this part of botany.

Why I have troubled the reader with this long and I fear tedious account has been mentioned: without a minute detail it is impossible, to clear up the innumerable mistakes, that have involved this part of botany in one cloud of errour; nor can they be too soon rectified. The first I shall notice is the supposing the *holders of the seed* (or those parts which retain the lobes, and fasten them to the embryo plant) to be the commencement of the radicle; an explanation universal among phytologists. But this mistake could never have been made, if the seed had been dissected *progressively.*

The holders of the seed taken for the commencement of the radicle.

Formation and growth of the seed.

The corculum, which is the first part formed of the embryo, as I have already shown, is the centre of the vessels; the stem and cotyledons shoot from the lower part in the pocket; the radicle from the other end; while the line of life, or impregnating duct, runs through it, in one undeviating thread. But instead of this simple progress it is said, that the radicle, avoiding this direct line, shoots from two different spots in the seed. How is the vessel, that must accompany it, to get there? As well might the tail and hind legs of a chicken be supposed to proceed from the string that fastens it to the egg; nor are the holders of half the consequence the string is to the bird, for that is the vehicle of nourishment to it, but the holders are merely an elongation of the seed vessel for the purposes before mentioned. The seed is merely a box where life is sheltered, but which is only kept from decay by the living embryo it contains; nor is it linked to its cradle till the last epoch; when the holders lengthen, and fasten themselves to the embryo plant, the cuticle enclosing it about an inch down the root, in order more securely to retain it. But as there are few seeds that are thus formed, and the lobes themselves with their outward cuticle *embrace the embryo* in almost all plants, it may well be believed the *part* can be but of *little consequence.* I know but one order, that gives the least sanction to the idea, and that is the grasses; and the cause of this appearance arises from the radicle occupying not above half the width of the corculum on which it is set; while the nourishing

Grasses.

ing vessels, taking advantage of this, swell out at the other side, giving the whole so crooked an appearance in many species as to be favourable to the supposition. But an unanswerable argument against it is, that not one of the vessels, the radicle must contain, in order to perform its various offices of secretion, impulsion, &c., is to be found in the holders, but all in the heart. This is truly the seat of life, or vital part of the embryo, for the time of its infancy; whereas the holders are a mere elongation of the seed lobes (which is easily proved by dissection) and a thick, strong, dry skin, till the nourishing vessels run on them; when they appear more moist, and increase in length very greatly about the time the embryo leaves the seed. But the view of the drawings will prove the mistake sooner than all my arguments perhaps; and that I may not be accused of favouring my subject by my sketches, I shall borrow one from that excellent work of Dr. Smith's, it is an exact figure of the bean, and plainly shows the holders in their proper light. Dr. Smith not being his own dissector will account for his being also implicated in the mistake, for he marks the holders as the *beginning* of the *radicle*. Mirbel and Wildenouw were also of this opinion. Duhamel, after calling them by a name synonimous to holders, seems to forget it, and finishes by marking them as the commencement of the root. Discouraged at finding so many great men against me, I had scarcely the resolution to seek in Grew for his opinion; and was really *delighted* to find, that he thought the holders of so little consequence, as but just to mention their retaining the lobes; without giving any reason for it. But no person can be deceived who will take a peach, cherry, bean, grass, or any kind of seed, and draw off the lobes; for they will find no resistance to the separation; and a piece of thin skin will be seen to have covered the radicle a little way down, and be very easily divided from it, breaking no vessels whatever.

The holders.

Mistaken for the beginning of the radicle by most botanists,

except Grew.

The next mistake I shall endeavour to rectify is that, which supposes the cotyledons to nourish the young plant; without recollecting, that they are a part of the embryo, and cannot therefore nourish themselves: a system absolutely contrary to the laws of nature. What, on this supposition, would

Cotyledons erroneously supposed to nourish the young plant.

would be the use of the nourishing vessels, which occupy towards the last epoch of the seed so large a portion of it, as plainly to evince their consequence? And what should cause these vessels to remain attached to the embryo, but the accidents to which the root is liable? We know indeed, that there is a spot in the seed, which adds to the juices of the nourishing vessels that *sweet fluid* requisite to the support of the young plant. In these, when mixed together, it reposes, as in a bath, sucking them in at every pore: but this sweet fluid alone would not suffice; a less cloying liquid is wanting, and *this* these *important nourishing vessels* produce. Where then is the use of further support? The cotyledons are often extremely full, juicy, and thick; and (especially in seed leaves) grow extremely fast: consequently, instead of nourishing other parts, they require for their own growth much support. In firs, where there are so many cotyledons (as almost all the pines have 8; indeed I know but two that have 4 only; though they do not all come to perfection), it must require a quantity of juices to form them, instead of assisting to form the embryo. Nor could I ever perceive any diminution in the cotyledons, though I have watched them with the greatest care. When they leave the seed, they are just as thick as ever, and altered only in their green colour; whereas the spot in the seed, which produces the sweet fluid just mentioned, shows, by the time the embryo leaves the seed, so large a vacancy, as plainly to indicate, that, if the embryo did receive nourishment from the cotyledons, these delicate leaves would proclaim the charity in a very conspicuous manner, having little of their *proper form* after such a reduction. Besides, in those seeds, where it was possible to do it without destroying the primordial leaves, which always greatly hurts the embryo, such as the orange, lemon, &c., I have repeatedly severed the cotyledons the moment they were formed, and it produced no visible effect; though, if it had in the least lessened the food of the embryo, so little can it bear such a privation, it would have died directly.

Nourishing vessels, reproduced as often as cut off,

There are innumerable convincing proofs of the power of the nourishing vessels, and one of the strongest is, that you cannot deprive the seed of them, for they increase as fast as you

you cut them off. The quantity of hairs that will replace a dilapidated piece in one night is really wonderful. Here therefore Nature plainly speaks her purpose: nor does she less pointedly make it known, when the nourishing vessels decay as soon as the radicle enters the earth. It is such indications as these of the laws and customs of nature, that should be collected by Botanists with care, after being thoroughly verified, and form their axioms, for they cannot mislead. But those which place nature in an unnatural situation, in order to ascertain her rules, I would ever reject; or keep them for trials only, and not build systems on them: for, if the foundation is not secure, how can you trust to the building? I shall say no more on this subject: as a little consideration must I think show, that the idea of the cotyledons nourishing the embryo is a mistake, which will I trust be rectified. I shall proceed therefore to my last subject, grieved that my letter has unavoidably spread to such a length.

but decay when the seed takes root.

To the division most physiologists make of monocotyledonous and dicotyledonous plants I should have no manner of objection, were it not founded on the mistaken supposition of there being plants, that have only one cotyledon, which is undoubtedly false. The palms, at least all I have been able to procure for dissection, have 2, and the grasses either 2 or 4. The orchises are so very diminutive in their seeds, that it is not easy for any one to dissect them; but I have been fortunate enough to detect one in a state, that showed its cotyledons in the solar microscope. It is a seed leaf, and has two. The only mosses I have been able to dissect on the same account have visibly two little round leaves from the interior of the pocket, being the usual place; whereas the grasses and palms have their cotyledons rising from the side of the heart, instead of the middle; and what has been taken by botanists for the cotyledons is the primordial leaf, which must of course in both those plants be single, since they grow only leaf by leaf. In the palm, (see Pl. II, figs. 27, 28, 30,) the cotyledons surround the heart, and are indeed very difficult to be separated from it. Nothing but boiling will do, and then it must be the corculum alone that is boiled. The leaves then peel off, and show their number.

The division of plants into monocotyledonous and dicotyledonous a mistake, as there are none with a single cotyledon. Palms Orchises.

Date palm.

In

In the part from which the pocket stretches the primordial leaf shoots, at least in the phœnix dactylifera. There are some very extraordinary things belonging to this seed: it has the appearance of being formed of a collection of extremely diminutive palm-leaves, coagulated and pressed together into so hard a substance, that, when cut into very thin slices with the *wood-cutter*, it presents a picture of pieces of palm leaves with all their veins and vessels. It is a long seed, and rolled, with a deep incision down the middle; and having the appearance of a very diminutive leaf, but very broad, opposite to the corculum, which lies also in the middle of the seed, a very unusual circumstance. See Figs. 26, 27.

Little wild palm.

The corculum is uncommonly large and white. The little wild palm has a still larger heart; the seed is round; it has two cotyledons spreading round it in the same manner as the other palm; and the wax palm has also two cotyledons, but thinner, whiter, and more delicate. I have planted them, and hope in my next to show their appearance when leaving their seed, which, as they require time, I have not yet been able to do.

Grasses.

As to the grasses, the reason that all physiologists have joined in refusing the name of cotyledons to those diminutive leaves, which have all the appearance of seminal leaves, and *certainly* perform *all the functions of them*, such as screening the primordial leaves, &c., has been, I suppose, because, instead of appearing at the middle of the bottom part of the cotyledons, they appear at the two sides; but they are undoubtedly the cotyledons, and the leaf, which has falsely been called so, is the primordial leaf, and proves itself to be *this*, by showing a complete grass leaf, exactly the same as those which succeed it. The cotyledons are diminutive, thin, silvery leaves, that screen the primordial ones; and should, I think, be restored to their original denomination.

Division of plants.

There can be no doubt, that the division of plants is an excellent one; but it would be quite as good, when founded on the primordial leaves being single, as the cotyledons being so. I have had some thoughts of arranging the seeds in such a manner, that a word or two added to the present general description should indicate what sort of seed the plant

plant had; whether a *leaf seed*, a *rolling seed*, or a *common seed*; making these the order, with genera and species, exactly to discriminate the sorts: for is it proper, that the exterior of seeds should be described in so elaborate a manner, and that the *interior*, by far of the most consequence, that part which Nature has distinguished with every attention and every care possible, should be wholly neglected? Would it not be extremely curious to inquire what effect a plant derives from being a *seed leaf?* For, though a seed leaf begins exactly like a common seed, and has all the eight parts before mentioned, it differs very greatly in one respect; that is, when the pocket is complete, and joins the heart, the cotyledons grow with a quickness impossible to describe, and have also additional means of nourishment for this purpose, and for the growth of those vessels, which, like common leaves, are regularly wove, elongating from the bark, and brought for this purpose from the exterior. This must cause a great change in the plant, I should conceive. Nor would it be less curious to see the effect of the *rolling seed* on the plants. But I shall leave these subjects to be discussed in my next, should this be received with the same degree of favour I have before been honoured with.

I am, Sir,

Your obedient humble servant,

AGNES IBBETSON.

P. S. Most succulent herbaceous plants have leaf seeds; most strong and vigorous trees have *common seeds*; and the rolling seed, which is a different sort of leaf seed, generally indicates a weak small plant, such as climbers, and creeping plants. I again repeat, it makes little difference in the seed, nor is it possible to tell what the seed will be, till nearly the end of the second period; but this difference I shall explain in my next. I have now dissected eight different sorts of mosses, and they have all two seminal leaves, and so have the tremella and the lichen. Division of seeds.

Explanation

Explanation of the Plates.

Explanation of the plates.

Plates I and II. Fig. 1. The seed vessel of the horse chestnut, cut open horizontally. *a*, *a*, *a*, *a*, the impregnating vessel, or vessel of life, proceeding from the interior of the seed vessel. *k*, *k*, the string that stretches to attain the heart, and which, I think, is mistaken for the radicle. *b*, *b*, *b*, *b*, the nourishing vessels (marked by dotted lines only, to distinguish them), which always proceed from the exterior of the seed vessel. *d*, *d*, *d*, *d*, the pocket.

Note. Similar letters of reference denote the same parts in all the figures.

Fig. 2. Half a similar seed vessel cut longitudinally, showing the first appearance of the seed vessel in the bud of a female flower.

Fig. 3. The seed of the horse chestnut, Fig. 4, that of the peach, and Fig. 5, that of a grass, as they first appear in the bud of the flower. *c*, the corculum. *e*, the cuticle, or outer skin of the kernel. The seed of the peach is delineated in the seed vessel; the others are taken out of it.

Figs. 6, 7, 8. The same seeds in their second stage of growth.

Figs. 9, 10, 11. The three seeds in their third stage, when impregnated; the pocket joining the corculum, and the string *k* disappearing.

Figs. 12, 13, 14. The seeds in their fifth stage: the corculum perfected, the seminal leaves almost complete, and the nourishing vessels on both sides of the seeds.

Figs. 15, 16, 17. The three plants, showing only the embryo of the chestnut and peach. This is now as complete as it ever is, till placed in the ground in the seed. It is given thus, to show, that there is no radicle to it; and that the root, which will grow as soon as it is placed in the ground, can proceed only from *x*. This is easily seen, by comparing these with the plants where the root is annexed, which is merely filled out, and grown longer; and where the heart is still to be found, marked by a dotted circle. *g*, *g*, the cotyledons. *i*, *i*, the primordial leaves.

Figs. 18, 19. The bean and its embryo from Dr. Smith. *g*, the cotyledons. *h*, the holders; which show how little they

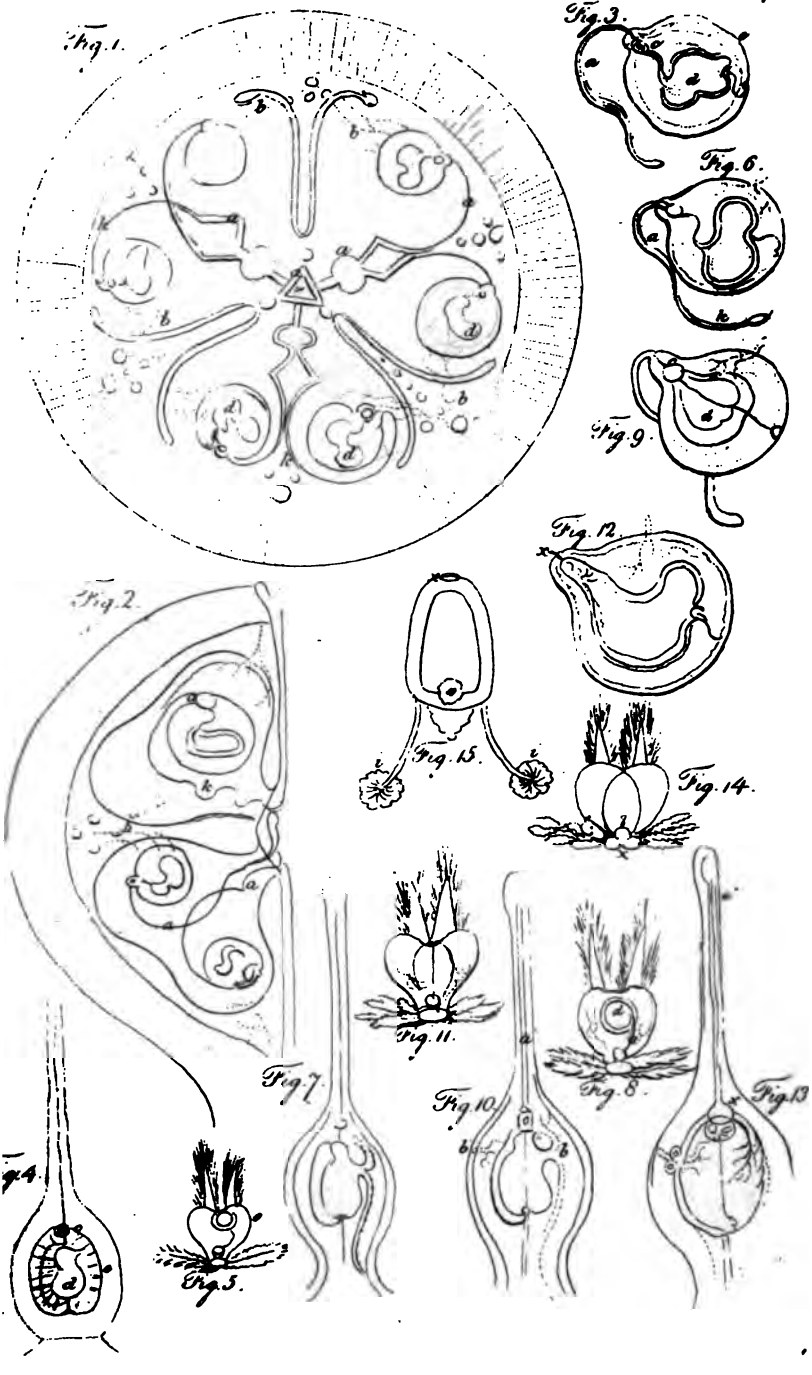
Fig. 1.
Fig. 3.
Fig. 6.
Fig. 9.
Fig. 12.
Fig. 2.
Fig. 15.
Fig. 14.
Fig. 11.
Fig. 7.
Fig. 8.
Fig. 10.
Fig. 13.
Fig. 4.
Fig. 5.

Fig. 25.
u
h
Fig. 30.
Fig. 24.
u
h
f
Fig. 29.
Fig. 23.
h
u
Fig. 18.
Fig. 27.
Fig. 19.
h
Fig. 22.
h
u
Fig. 28.
Fig. 16.
Fig. 17
Fig. 21.
h
Fig. 26.
Fig. 20.
h

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they can pretend to be the origin of the root. This is farther evident from

Figs. 20 and 21: the former showing a French bean, with the part to which the radicle grows starting from it: the latter, the same magnified: *x*, the part on which the cotyledons grow; *y*, that from which the radicle proceeds; *h*, *h*, the holders.

Figs. 22, 23, 24, 25. The chestnut, peach, grass, and bean; showing the completion of the embryo by its growing in the earth. *f*, the radicle. *u*, the seed.

Fig. 26. The seed of the date palm.

Fig. 27. The heart greatly magnified, with the leaves, or cotyledons, wrapped round it.

Fig. 28. The same, with the leaves unfolded, to show that they are two, and that the point leaf is a primordial leaf.

Fig. 29. The seed of the little palm.

Fig. 30. The heart, with its two cotyledons unfolded.

II.

On the Ratio the spontaneous Evaporation of Water bears to Heat: by HONORE FLAUGERGUES*.

THE celebrated academy at Lyons proposed last year as the subject of a prize, "to determine the relation between the spontaneous evaporation of water and the state of the air, as shown by the thermometer, barometer, and hygrometer." This interesting question I was tempted to investigate; and accordingly I began a series of experiments as early as the month of September, 1806, which I have since continued without interruption. The academy was very indulgent to the paper I had the honour of transmitting it on this subject: but the prize it condescended to bestow on me I consider less as a reward, which I was far from meriting, than as an excitement to multiply and extend my researches. I have therefore continued the inquiry I had begun, so that my work has reached a considerable extent; Prize question proposed.

* Journal de Physique, vol. LXV, p. 446.

and as it cannot be published entire, I have thought I should gratify the lovers of natural philosophy, if I extracted from it what relates to the relation between evaporation and heat, giving it in as concise a form as possible.

Is evaporation proportional to the surface exposed simply?

Before I entered into the particular investigation of the changes, which the state of the air occasions in evaporation, I thought it would be right to examine the general law, which it follows in all cases, and endeavour to decide the grand question, whether, as most philosophers think, evaporation be proportional to the extent of surface of the water in contact with the air; or whether it depend also on some function of the other dimensions of the body of water exposed to evaporation, as Muschenbroeck* and Côte† assert.

Under similar circumstances it is.

With this view I have made a number of experiments; and, after having varied them in all ways, I have constantly found, that, under similar circumstances, evaporation is precisely proportional to the extent of the surface of the water in contact with the air. I found too, that, when these two gentlemen imagined they had observed the existence of another law, it was because, from the arrangement of the vessels employed in their experiments, the water contained in them was heated and cooled unequally, whence arose accidental variations of the evaporation, concealing the true law; and which would not have taken place, if these vessels had been placed in air of a constantly uniform temperature, or if they had been surrounded with a large body of earth, or some other substance, as Mr. Sedilleau long ago observed ‡.

Measure of evaporation.

When we find, that evaporation, under similar circumstances, is proportional to the surfaces, we require nothing more, to express its measure, than the number of lines the surface of the water is lowered by this evaporation in a given time. For this time I have taken four and twenty hours, or one day.

* Essays on the Experiments of the Academy del Cimento, Tome I of the Academical Collection, part. 6tr. p. 142.

† Journ. de Physique, vol. XVIII, p. 306.

‡ Anc. Mém. de l'Acad. des Sciences, tom. X, p. 33

The

The experiments I made to determine the relation of evaporation to heat were very simple. On a table in the middle of a large room, the air of which was perfectly still and heated to a given temperature, and the humidity of which was also ascertained and constantly the same, I placed cylindrical or prismatic vessels of glass and metal, the diameters of which were of no importance; but I made them all above an inch, because evaporation goes on less freely in vessels with small apertures. These vessels I filled with spring water, heated precisely to the temperature of the room, and noted the time when the experiment commenced. Keeping the air of the room uniformly at the same temperature, when I thought the quantity of water evaporated might begin to render the air sensibly moister, and thus diminish its solvent power, I measured how much the surface of the water in the vessels was lowered. The time of taking this measure, which was that of the conclusion of the experiment, I noted down; and by the rule of proportion I found what the evaporation would have been, if the experiment had continued twenty-four hours. Experiments to ascertain it.

These experiments, though very simple, are attended with some difficulty, if well executed. It is not easy, to keep the air of a large room at the same temperature for any length of time, or to have it always at the same degree of humidity. By care, however, I accomplished both these points for a time sufficient to be perfectly sure of the results of these experiments. Difficulties.

To determine the degrees of heat I employed several excellent thermometers, constructed on the principle of Mr. De Luc, and two of which were made by the late Mr. Paul of Geneva. I was not equally happy in my means of ascertaining the degree of humidity; for whatever pains I took, I could not procure one of Mr. Saussure's hair hygrometers. This instrument, considered as the most accurate, or least defective, of the kind, is very difficult to be met with since the death of this celebrated artist, who was perhaps the only person that succeeded in making good ones. But as all that was requisite in the present instance was to find one constant degree of humidity, I endeavoured to supply their want by means of hygrometers made of one Instruments employed.

piece of gut, but which I constructed with particular attention, as I may perhaps relate at large on some future opportunity. I shall only say, that the uniform degree of moisture I chose for my experiments answered nearly to 50° of de Saussure's hygrometer*.

Measurement. To measure the lowering of the surface of the water in the vessels, I employed a scale of 1000 equal parts, accurately divided by Canivet. Of these parts 190 were precisely equal to a French inch. I took the measure on the side of the glass vessels with a pair of spring compasses, the points of which were extremely fine, and a lens. For those of metal I employed a small commodious instrument. It consists of a capillary tube of glass, firmly fastened at right angles to a wooden ruler perfectly straight; and a scale similar to the preceding traced on a very thin and narrow slip of brass, fixed to the tube. The inside of the tube being wetted by a drop of water introduced into it, the tube is immersed perpendicularly in the water of the vessel, till the edge of the ruler rests on the brim of the vessel. The water ascends in the tube by capillary atraction, and the point of the scale to which it rises at the commencement of the experiment is noted down. The same operation is repeated at the conclusion of the experiment; and the difference between the former point and that to which it now rises measures the lowering of the surface by evaporation.

Law of evaporation. When I had thus obtained five or six well defined evaporations, corresponding to equal intervals expressed in degrees on the scale of the thermometer, or to equal differences of heat, I endeavoured to find the law these evaporations followed. For this purpose I made many fruitless trials, till an idea suggested itself, which from its simplicity ought to have presented itself at first; that of introducing between the two extreme evaporations as many geometrical proportionals as there were evaporations observed between

* In my experiments on the ratio of evaporation to the moisture of the air I employed a method of determining this moisture more certain than the most perfect hygrometers; that of calculating directly the quantity of water in vapour contained in a given bulk of air, by absorbing this water by perfectly dry potash.

them.

them. Having done this, I found to my great satisfaction, that these mean geometrical proportionals evidently represented the intermediate evaporations. All the experiments I afterward made, the results of which are given in the following table, confirm this law.

The first column of this table contains the degrees of Mr. De Luc's thermometer*, at the temperature of which I made my experiments†. Explanation of the table.

The third column contains the mean results of 291 experiments, which I made to determine the ratio of evaporation to heat, and the degree of spontaneous evaporation of water at every degree of Mr. De Luc's thermometer from 0° to 31°. To give the particular result of every experiment would have been attended with little advantage, and occupied too much valuable room: accordingly I have divided the sum of the evaporations observed in every experiment under the same degree of heat by the number of these experiments, and given only the quotient, or mean result.

The fourth column contains the evaporations calculated according to the rule above mentioned; that is, by inserting 30 mean geometrical proportionals between the numbers expressing the evaporations observed at 0° and 31°.

The fifth column gives the difference between the evaporations thus calculated and the mean of those observed.

In making these experiments I chose times when the barometer was about its mean height, which I determined by 1000 observations at my observatory, each day at noon, to be 27 inches, 9·3 lines [29·56 in.], supposing the quicksilver at the temperature of melting ice.

* A second column is here added, containing those of Fahrenheit. C.

† I made some other experiments indeed, and as far as 40° of the thermometer [122° F]; but as I have not yet been able to repeat them as often as I wished, I have not reported their results.

Thermometer.

Table of the spontaneous evaporation of water at different temperatures.

Thermometer.	Fahrenh.	Daily evaporation observed.	Daily evaporation by the rule.	Difference.	Thermometer.	Fahrenh.	Daily evaporation observed.	Daily evaporation by the rule.	Difference.
0°	32°	4·4	4·4	0	16°	68°	17·8	18·7	+ 0·9
1	34·25	4·5	4·8	+ 0·3	17	70·25	19·4	20·5	+ 1·1
2	36·5	4·4	5·3	+ 0·9	18	72·5	22	22·4	+ 0·4
3	38·75	5·9	5·8	— 0·1	19	74·75	24·8	24·6	— 0·2
4	41	6·8	6·3	— 0·5	20	77	27·3	26·9	— 0·4
5	43·25	7·3	6·9	— 0·4	21	79·25	30·2	29·4	— 0·8
6	45·5	7·4	7·6	+ 0·2	22	81·5	32	32·2	+ 0·2
7	47·75	8	8·3	+ 0·3	23	83·75	35·6	35·2	— 0·4
8	50	9·6	9·1	— 0·5	24	86	38·9	38·6	— 0·3
9	52·25	10·3	9·9	— 0·4	25	88·25	42	42·2	+ 0·2
10	54·5	10	10·9	+ 0·9	26	90·5	46·8	46·2	— 0·6
11	56·75	10·9	11·9	+ 1	27	92·75	51	50·6	— 0·4
12	59	13·2	13	— 0·2	28	95	55·7	55·4	— 0·3
13	61·25	14	14·3	+ 0·3	29	97·25	61	60·7	— 0·3
14	63·5	15·9	15·6	— 0·3	30	99·5	66·9	66·4	— 0·5
15	65·75	16·4	17·1	+ 0·7	31	101·75	72·7	72·7	0

The differences owing to inaccuracies in the experiments.

If we examine the differences in the fifth column, we shall readily perceive, 1, that these differences are very small; 2, that they are indifferently positive or negative; and, 3, that their sum is next to nothing: whence it follows, that these slight differences may reasonably be ascribed to the errours unavoidable in experiments of this kind; and that, without these errours, the evaporations observed would have coincided with those calculated according to our hypothesis, which may consequently be considered as perfectly conformable to nature.

Evaporation increases in a geometrical progression.

From the preceding experiments therefore we may infer this remarkable law, that, while the degrees of heat increase or diminish in arithmetical progression, the corresponding evaporations increase or diminish in geometrical progression. Thus, the heat in our experiments increasing uniformly and successively one degree [2·25° F.], the corresponding evaporations form a geometrical progression, each term of which is to the preceding in the ratio of 1·0947 to 1. So these evaporations form a geometrical progression nearly in

in a duplicate ratio, if we take intervals of 7·6° [17·1° F.]; and nearly in a triplicate ratio, if the intervals be of 12° [27° F.].

Hence it follows, if we suppose, that the degrees of the thermometer are represented by equal parts of a right line, and that on each of the points corresponding to a degree we erect a perpendicular equal to the evaporation that answers to that degree of heat, the degrees of the thermometer will be the abscisses, and the corresponding evaporations the ordinates, of a logarithmic curve, the subtangent of which may be found by the following ratio. Formula.

As 2·8047369 (the difference of the Naperian logarithms 1·4816045 and 4·2863414, answering to the numbers 4·4, and 72·7) is to 31 (the difference of the correspondent abscisses 0 and 31), so is 1 (the subtangent of the logarithmic of the Naperian system) to 11·0527301 (the subtangent of the logarithmic of the evaporations).

The equation of the logarithmic, putting x for the abscisse, y for the ordinate, and S for the subtangent. is $S\,dy = y\,dx$. If we sum up this equation; complete the integral, remembering, that $x = o$ gives $y = \log.\ (4{\cdot}4)$; and reduce it to numbers, putting for S the value found above; we shall ultimately have the equation

$$y = (4{\cdot}4) \,.\, (2{\cdot}7182818)^{\frac{x}{11{\cdot}0527301}}$$

In which equation x represents the degree of Mr. De Luc's thermometer given, and y the corresponding evaporation expressed in parts of my scale of 1000 equal parts. If we would have the evaporation in millimetres, this value may be multiplied by $\frac{27{\cdot}07}{1000}$, or the number 0·6268843 may be substituted in the equation instead of the coefficient 4·4 *.

From the nature of the logarithmic, if we suppose dx constant, we shall have dy proportional to y: whence we may infer, that, the increments of heat taking place by in- Property leading to a knowledge of the nature of evaporation.

* To have the evaporation in English inches, this value should be divided by 178·272, the number of parts in the scale of Mr. Flaugergues equivalent to an English inch; or 0·0246812472 substituted instead of the coefficient 4·4. C.

finitely

finitely small and equal degrees, the corresponding increments of evaporation are proportionals to the evaporation itself; a singular property, and which, it seems to me, may lead to a more accurate knowledge of the nature of evaporation, and decide between the two celebrated systems of Leroy and Dalton, which at present divide the suffrages of natural philosophers.

III.

Method of ascertaining the Value of Growing Timber Trees at different and distant Periods of Time: *By Mr.* CHARLES WAISTELL, *of High Holborn**.

SIR,

Method of ascertaining the value of growing timber.

CONCEIVING, that the Tables contained in the annexed papers will afford useful information to growers of timber, and tend to encourage the growth of it in these kingdoms, and hereby promote the views of the Society of Arts &c., I trust you will have the goodness to lay them before the Society, as I have formed them with great attention.

Having last autumn viewed some plantations made under my direction about thirty years ago, I found the value of one of them much to exceed my expectation. I became therefore desirous to devise some means of estimating what its value might probably be at different future periods. I was thus led to construct the first of these tables, and on the completion of this, other tables seemed necessary, and I was thus progressively led on to the construction of the whole. For this purpose I searched in various authors for the measure of trees, in girth and height, at different ages, and obtained similar information among my acquaintance. Hence I collected, that the increase in the circumference of trees is generally from about one to two inches annually, and

General increase of trees in height and girth

* Trans. of the Soc. of Arts, vol. XXVI, p. 45. The gold medal of the Society was voted to Mr. Waistell for this communication.

from

from twelve to eighteen inches the annual increase in height. Some fall a little short, and some exceed these measures.

I shall now briefly notice a few of the advantages to be derived from the first Table. Use of the first table,

1st. The first table shows every fourth year, from twelve years old to a hundred, the rates per cent per annum at which all trees increase, whether they grow fast or slow, provided their rate of growth does not vary. This table may be the means of saving young thriving woods from being cut down, by showing how great a loss is sustained by felling timber prematurely*. trees should not be felled too soon,

2d. And it may be the means of bringing old trees to market, by showing the smallness of the interest they pay for the money they are worth, after they are 80 or 100 years old. or stand too long.

But this table shows the interest which they pay, only, while the trees continue growing at their usual rate. In case they fall short only a little of their usual increase in girth, this considerably diminishes the rate per cent per annum of their increase. And trees do decrease in their rate of growth, before they appear to do so†. A pale and mossy bark are certain indications of it. Trees decrease before it is apparent. Signs of this.

* "A wood, near West Ward, in Cumberland, of more than 200 acres, was felling in 1794, it was little more than 30 years old. The whole was cut away without leaving any to stand." *See Miller's Gardener's Dictionary, last edition, under the Head of Woods.*

At 30 years old timber pays 10 per cent for standing, and probably this wood might have paid 7 per cent per annum on an average for the next 30 years.

† In Mr. Pringle's Agricultural Report for Westmoreland is a paper of the Bishop of Llandaff's, stating, "That a very fine oak, of 82 years growth, measured in circumference at 6 feet from the ground, on the 27th of October 1792, 107 inches, and on the same day of the same month in 1793 it measured 108 inches." He then states the interest it paid to be only about 2 per cent, and says this tree was a singularly thriving one. It is evident, that, with all this appearance of thriving, it was on the decline. For if we divide 108, its inches in circumference, by 82, its age, we find its average annual increase had been 1 inch and a third. Its falling off to 1 inch reduced the rate per cent of increase one fourth.

3d.

3d. The first Table may also assist the valuer of such timber as is not to be cut down, but to continue growing, by enabling him to estimate its present value more accurately than is usually done, especially when it is increasing after a high rate per cent per annum*.

2d table. The 2d Table shows the rate per cent to be the same as in the first Table, though the annual increase is more both in height and circumference.

3d table. Distance of trees. The 3d Table is calculated to show the number of trees that will stand on an acre of ground, at the distance of one fifth of their height, (which distance is recommended by Mr. Salmon, in a paper in the Society's 24th volume,) and the number of feet the tree will contain, both those to be cut out, and those to be left standing, at the end of every four years, from 16 to 64 years old, supposing they increase 12 inches in height and 1 in circumference annually. This distance may suit fir trees, but will be too near for oaks.

4th and 5th tables. The 4th and 5th Tables show the same particulars when the trees grow at greater rates.

6th table. The 6th Table is calculated to show the same particulars when the trees are constantly thinned out every four years, so as to leave them at the distance of one fourth of their height. According to this table there will be 48 trees left on an acre when they are 120 years old; and it seems generally agreed, that from 40 to 50 full grown oak trees are as many as have sufficient room to stand on an acre.

7th table. The 7th Table shows the same particulars respecting trees which increase 15 inches in height and $1\frac{1}{2}$ inch in circumference annually.

8th table. The 8th Table shows the same particulars respecting trees which increase 18 inches in height, and 2 inches in circumference annually.

* A fir wood of more than 30 acres, and about 50 years old, was lately valued to be sold with an estate, by several eminent wood valuers, without taking into consideration its rate of increase. It was then increasing after the rate of 10 per cent per annum, and probably would increase after the rate of 8 per cent on an average for the next twenty years.

The

The 9th Table shows the same particulars as Table 6, till the trees are 28 feet high, after which the distance is increased from one fourth to one third of their height. 9th table.

The 10th, 11th, and 12th Tables show the annual increase in boles of 24, 32, and 40 feet long, and the difference of their increase at the same ages. 10th, 11th, & 12th tables.

To these tables succeed comparative statements, showing the number of feet contained in boles of different lengths, when the trees are 60 years old, by which it appears, that, if cut down at that age, the longest boles are not the most profitable to the growers of timber. Comparison of boles of different lengths.

And I have added the valuation of the plantations before alluded to, with remarks on them.

Having finished my introductory remarks, I conclude, and am, Sir,

Your very humble servant,

CHARLES WAISTELL.

Tables respecting the Growth of Timber.

Calculations, showing every fourth year, from 12 to 100, the progressive annual increase in the growth of trees, and gradual decrease in the rate per cent per annum, that the annual increase bears to the whole tree. Tables respecting the growth of timber.

The whole height of the trees is taken to the top of the leading shoot, and the girt in the middle; but no account is taken of the lateral branches.

If trees increase 12 inches in height and 1 in circumference annually, their increase will be as follows, viz.

TABLE

TABLE I.

Years old & ft. high.	Girt.	Contents.			Years old & ft. high.	Girt.	Contents.				One year's increase.				Rate per cent of increase.
	inch.	ft.	in.	pts.		inch.	ft.	in.	pt.	sds.	ft.	in.	pt.	sds.	
12	1½	0	2	3	13	1⅝	0	2	10	3	0	0	7	3	26·8
16	2	0	5	4	17	2⅛	0	6	4	9	0	1	0	9	19·9
20	2½	0	10	5	21	2⅝	1	0	0	8	0	1	7	8	15·7
24	3	1	6	0	25	3⅛	1	8	4	1	0	2	4	1	13·
28	3½	2	4	7	29	3⅝	2	7	9	1	0	3	2	0	11·
32	4	3	6	8	33	4⅛	3	10	9	6	0	4	1	6	9·67
36	4½	5	0	9	37	4⅝	5	5	11	5	0	5	2	5	8·5
40	5	6	11	4	41	5⅛	7	5	8	10	0	6	4	10	7·6
44	5½	9	2	11	45	5⅝	9	10	7	9	0	7	8	9	6·96
48	6	12	0	0	49	6⅛	12	9	2	3	0	9	2	3	6·38
52	6½	15	3	0	53	6⅝	16	1	10	2	0	10	10	2	5·9
56	7	19	0	8	57	7⅛	20	1	1	7	1	0	5	7	5·4
60	7½	23	5	2	61	7⅝	24	7	6	6	1	2	4	6	5·1
64	8	28	5	4	65	8⅛	29	9	7	0	1	4	3	0	4·76
68	8½	34	1	4	69	8⅝	35	7	8	11	1	6	4	11	4·49
72	9	40	6	0	73	9⅛	42	2	6	4	1	8	6	4	4·2
76	9½	47	7	6	77	9⅝	49	6	5	2	1	10	11	2	3·98
80	10	55	6	8	81	10⅛	57	7	11	9	2	1	3	9	3·79
84	10½	64	3	8	85	10⅝	66	7	7	8	2	3	11	8	3·6
88	11	73	10	4	89	11⅛	76	5	11	1	2	7	7	1	3·5
92	11½	84	5	9	93	11⅝	87	3	4	0	2	9	7	0	3·3
96	12	96	0	0	97	12⅛	99	0	4	6	3	0	4	6	3·15
100	12½	108	6	0	101	12⅝	111	9	6	8	3	3	6	8	3·

Remarks. In Table X of the increase of a bole of 24 feet in height, of a tree growing at the abovementioned rate, it will be observed, that the contents at 24 years of age are the same, and at 64 years nearly the same as in the above Table, but the contents of the bole at all the intermediate periods exceed the above. And a 40 feet bole exceeds the above contents from 44 years to 100, as may be seen in Table 12. For these reasons chiefly I did not think it necessary to take into consideration the decrease in height that takes place in trees at different ages, according to the kind of tree and quality of the soil.

The increase per cent per annum is the same as the above in all trees at the same age, whether they have grown faster or slower, provided their increase in height and thickness annually has not varied on an average. The progress of trees is sometimes greatly retarded by insects destroying their

their leaves, by unfavourable seasons, and by their roots penetrating into noxious strata. But these accidents cannot enter into calculations.

Calculations, showing every fourth year from 12 to 64, the progressive annual increase in the growth of trees, and the gradual decrease in the rate per cent per annum that the annual increase bears to the whole tree. Table 2.

The whole height of the trees is taken to the top of the leading shoot, and the girt in the middle; but no account is taken of the lateral branches.

If trees increase eighteen inches in height, and two inches in circumference, annually, their increase will be as undermentioned, viz.

TABLE II.

Age of Trees.	Height.	Girt.	Contents.			Age of Trees.	Height.	Girt.	Contents.				One year's increase.				Rate per cent of increase.
	ft.	in	ft.	in.	pt.		feet.	inch.	ft.	in.	pt.	sd.	ft.	in.	pt.	sd	
12	18	3	1	1	6	13	19½	3½	1	5	1	0	0	3	7	0	26·5
16	24	4	2	8	0	17	25⅔	4⅔	3	2	4	0	0	6	4	0	19·8
20	30	5	5	2	6	21	31½	5½	6	0	3	6	0	9	9	6	15·6
24	36	6	9	0	0	25	37½	6½	10	2	0	6	1	2	0	6	13·
28	42	7	14	3	6	29	43⅔	7⅔	15	10	6	0	1	7	0	0	11·
32	48	8	21	4	0	33	49½	8½	23	4	8	0	2	0	8	0	9·6
36	54	9	30	4	6	37	55⅔	9⅔	32	11	7	6	2	7	1	6	8·5
40	60	10	41	8	0	41	61½	10½	44	10	3	6	3	2	3	6	7·6
44	66	11	55	5	6	45	67½	11½	59	3	10	0	3	10	4	0	6·9
48	72	12	72	0	0	49	73½	12½	76	7	1	0	4	7	1	0	6·3
52	78	13	91	6	6	53	79⅔	13⅔	96	10	11	6	5	4	5	6	5·8
56	84	14	114	4	0	57	85½	14½	120	6	8	6	6	2	8	6	5·4
60	90	15	140	7	6	61	91½	15½	147	9	2	0	7	1	8	0	5·
64	96	16	170	8	0	65	97½	16½	178	9	4	0	8	1	4	0	4·7

Explanation of the Construction of Table I and II.

To render the preceding tables easy to be understood by persons not accustomed to calculations, I will state the process of the operations in the first line of Table II. Construction of tables 1 and 2 explained.

The height of the tree at 12 years of age is supposed to be 18 feet to the top of its leading shoot, and 24 inches in circumference

circumference at the ground, consequently, at half the height, the circumference is 12 inches. One fourth of this, being 3 inches, is called the girth. The girth being squared, and multiplied into the height, gives one foot one inch and six parts for its contents. At 13 years old the tree will be 19½ feet high, 26 inches in circumference at the ground, and 13 inches at half the height; one fourth of 13 gives 3¼ inches for the girth. This squared and multiplied into the height, give one foot five inches and one part for the contents. Deduct from this the contents of the tree at 12 years of age, and there remain three inches and seven parts, which is the increase in the 13th year. Then reduce the contents of the tree when 12 years old, and the increase in the 13th year, each into parts, dividing the former by the latter, and the quotient will be 3·76; by this number divide 100, and the quotient is 26·5, which is the rate per cent of increase made in the thirteenth year. Consequently whatever the tree might be worth when 12 years old, it will, at the end of the 13th year, be improved in value after the rate of 26*l.* 10*s.* per cent, or in other words, that will be the interest it will have paid that year for the money the tree was worth the preceding year.

At every succeeding period, both in this Table and Table I, the like process is gone through.

Observations on Tables I and II.

General observations on the preceding tables.

The preceding tables furnish us with the following useful information, viz.

1st. That all regular growing trees, measured as above, as often as their age is increased one fourth, contain very nearly double their quantity of timber.

2nd. That when a tree has doubled its age, its contents will be eight-fold.

3d. That when a tree has doubled its age, the annual growth will be increased four-fold.

4th. Consequently, that when a tree has doubled its age, the proportion that its annual increase bears to the contents of the whole tree is then diminished one half.

This

This last observation explains how it comes to pass, that a tree, when its age is doubled, the rate per cent per annum that its increase then bears to the contents of the whole tree, is diminished one half.

It may not be unuseful to observe, that the rate per cent of increase in the last columns, is the same as the rate per cent that the increase of the tree that year will pay for the money it was worth the preceding year.

In the two preceding tables, we find that the rate of increase per cent per annum is the same in both at the same ages, although the quantity of timber in the second table is six times as much as in the first table in trees of all ages; therefore, when the age of a tree is known, the rate per cent per annum of its increase is known on inspecting these tables, whether the tree has grown fast or slow; provided the growth of the tree has been regular, and that it has continued its usual growth.

And having the age, girth, and height of any tree given, we can readily calculate what quantity of timber it will contain at any future period, while it continues its usual rate of growth.

(To be continued.)

IV.

Observations on Potash and Soda prepared with Alcohol: by Mr. d'Arcet. *Read to the Institute the 11th of January,* 1808.*

WHEN chemistry, employing new methods of analysis, is enriching itself with important facts; when England announces the decomposition of potash and soda, and the chemists of France are busied in confirming this grand discovery; I conceive it incumbent on me to communicate the results of various experiments, which may probably throw some light on the path newly opened.

* Annales de Chimie, vol. LXVIII, p. 175.

I present

Importance of facts relative to the fixed alkalis.

I present only facts that still require verification; but they appear to me of the greater importance, as they relate to those alkalis, the decomposition of which has been announced; and are naturally applicable to the analysis of saline substances, an important branch of science, since almost all analytical processes ultimately produce them, and then conclusions are formed from the knowledge we have of the proportions of their elements.

Method of finding the quantity of alkali in the impure sorts of the shops.

Seeking some months ago an easy and speedy method of ascertaining the quantity of pure or carbonated alkali contained in the different sorts of potash and soda found in the shops, I compared the various processes that have been made public; and soon perceived the advantage of those, in which acids are employed to determine the quantity of alkali, and this quantity is found from the weight of acid required to neutralize the mixture.

Sulphuric acid preferred.

Various considerations, which it is unnecessary to mention, led me to prefer the sulphuric acid, as proposed by Mr. Descroizilles. I carefully examined his method, and, fully satisfied of its goodness, made the following experiments. I must observe, they were all made with at least 20 gr. [309 grs]; but most of them with 100 gr. [1544 grs]; and that each result is the mean of four experiments, which frequently differed only in the second decimal figure. I began by thoroughly purifying a few kilogrammes of subcarbonate of soda. After having separated by successive crystallizations the small quantity of muriate and sulphate of soda it contained, I reduced the crystals to coarse powder, and left them exposed to a temperature of 12° or 14° [54° or 57° F.], till they were thoroughly dry. I then took some sulphuric acid, carefully distilled, and very pure, the specific gravity of which was to that of water as 1·844 to 1: I reduced its specific gravity to 1·066 by diluting it with 9 parts of distilled water: and this acid, thus diluted, I employed in the course of my experiments. I need not say, that, on dividing its weight by ten, the corresponding quantity of concentrated acid is found, which, expressed in numbers, would represent the strength of the alkali employed to saturate it.

The author's method of proceeding.

Analysis of

These preliminaries being settled, the analysis of the subcarbonate,

carbonate, which I had prepared, was conducted with all possible care, varied in several ways, and uniformly announced its composition to be

subcarbonate of soda.

Water	63.61
Carbonic acid	16·04
Soda	20·35
	100.

Considering myself certain of the accuracy of these results, and having paid so much the more attention to the experiments that furnished them, because they were to serve as a standard to the rest, I thought I might take this analysis as a settled point, and then proceeded to the following experiments.

Taking the usual precautions, I first neutralized 100 gr. [1544 grs] of the subcarbonate of soda abovementioned. Repeating this experiment several times, the mean term of the results was 347, expressing the quantity of diluted acid required for the saturation, and representing 34·7 grammes of concentrated acid.

Quantity of sulphuric acid required to saturate it.

Thus I found myself authorized to conclude, that the employment of 34·7 grammes of concentrated sulphuric acid, similar to that of which I have given the specific gravity above, would always represent, at the same temperature, in a solution of soda brought by this acid to the neutral state, 100 grammes of subcarbonate of soda similar to that I had analysed; or, which is the same thing, 36·39 of dry subcarbonate, or 20·35 of pure soda.

I then repeated the same experiments, substituting, instead of the subcarbonate of soda, caustic soda prepared with alcohol, hitherto considered as pure soda, and the real standard of this alkali: but I was surprised at the results I obtained; and the conclusions I was compelled to draw appeared to me so contrary to the received opinions, that I omitted nothing, to remove every sort of doubt. Accordingly I made a number of experiments, and obtained the following results.

Soda purified with alcohol saturated with the same acid.

First I examined four different specimens of soda prepared with alcohol, and simply fused in a silver capsule:

none of these specimens were perfectly pure; all of them indicating slight traces of muriatic acid, and a greater or less proportion of carbonic acid, easily detected by barytic salts, barytes water, lime water, &c., but too little for a solution not very strong to effervesce on the addition of the acid. On neutralizing 20 gr. [309 grs] of each of these specimens, I found, that 100 parts of

Proportions required.

N° 1 had absorbed 110·2 of concentrated sulphuric acid.
2..... 116·75
3..... 111·5
4..... 112·2

This would indicate, taking the mean of the results, that 100 parts of caustic soda required only 112·662 of concentrated sulphuric acid for their neutralization.

The experiments repeated.

Apprehending, that the specimens of soda employed, notwithstanding they had been in fusion, might contain more or less water, I repeated these experiments on similar portions of soda, which had been fused separately in a silver crucible, and kept in this state at a red heat for twenty minutes: but the proportions obtained differed so little from those above given, that it is unnecessary to insert them.

Soda thus prepared therefore not real alkali.

On comparing these results with those before obtained, we must conclude, that, if 20·35 of pure soda, in the subcarbonate analysed, required 34·7 of concentrated acid to saturate them, 100 would take 170·515: but we have just seen, that 112·662 of this acid were sufficient to neutralize 100 of the caustic soda prepared with alcohol; whence it follows, that this soda was not pure, which is probable; or that the analysis of the subcarbonate was erroneous, a supposition that I conceive inadmissible, from the various trials made.

Experiments repeated with soda

Notwithstanding I was satisfied, that the four specimens of soda prepared with alcohol contained too small a portion of any known foreign matter, to occasion so great a difference between the results obtained, I thought it proper to repeat the same experiments with pure soda prepared in a different way.

purified with and without alcohol.

I took a kilogramme [35¼ oz avoird.] of perfectly pure crystallized sulphate of soda; decomposed it by means of barytes,

barytes, taking care to use a little excess; filtered; and evaporated quickly to dryness. Half of the residuum was put into alcohol, and treated as usual. The other half was dissolved in barytes water. The liquor, containing but a slight excess of barytes, was filtered, speedily evaporated, and fused in a silver crucible at a cherry red heat, as the portion prepared with alcohol had been.

The latter neutralized most acid.

Of these two specimens, 100 parts of that prepared with alcohol required 119·6 of concentrated acid for their saturation; and 100 of the other took 122·4 of acid. These results confirm the preceding; and appear to demonstrate, that the soda prepared with alcohol contains only 0·71 or 0·72 of such alkali, as in the subcarbonate and sulphate of soda is neutralized by carbonic and sulphuric acid.

Similar trials with potash gave similar results.

Similar experiments repeated in the same way, substituting for the soda caustic potash prepared with alcohol, and for the carbonate and sulphate of soda the corresponding salts with base of potash, afforded analogous results; and authorize me to conclude, that potash prepared with alcohol, far from being pure potash, contains only 0·72 or 0·73 of real alkali.

These facts require a revision of many analyses.

If these experiments be accurate, it follows, that potash and soda prepared with alcohol cannot be employed to ascertain by synthesis the proportions of the constituent principles of saline substances, that have these alkalis for their base. This is an important corollary, since it requires a revision of many experiments founded on this principle, in order to correct their results; or at least to confirm the alterations, that so great a difference in the principal datum must occasion.

Instances in Vauquelin's analysis of different sorts of potash,

Among the examples I might adduce, I shall select such as appear to me best fitted to establish the truth of this position. In the year 10 Mr. Vauquelin published an important essay on the analysis of different kinds of potash, and on the means of readily ascertaining the quantity of pure alkali in them. In this paper, which has already been so serviceable to the arts, both by its immediate application, and by giving birth to the researches of Mr. Descroizilles, the author, after having ascertained the quantity of nitric acid of a known density necessary to neutralize a given

quantity of potash purified by means of alcohol, offers this result as a term of comparison, and as a standard of the greatest possible purity of potash. This however would give a very erroneous computation of the quantity of real alkali, as it appears, that the potash taken as a standard contains only 0·73 of its weight of pure potash.

and ascertaining the component parts of salts.

It is more especially in determining the proportion of the constituent principles of salts, that this source of errour is to be carefully avoided; for we know how important a good solution of this problem would be, and how great the difficulties are, that have hitherto prevented our attaining it. Mr. Berthollet, in his inquiries into the laws of affinity, applying new methods of experimenting to this question, examined those employed by Richter and Kirwan in their labours on the same subject. He found, that Kirwan, beside the number of estimations he was obliged to make, had set out with a principle of too little accuracy; and to this he ascribed much of the uncertainty of the results this chemist obtained. Yet Kirwan, by employing solutions of subcarbonate of potash and of soda, to ascertain the proportions of the salts that have these alkalis for their base, had only to apprehend the slight errour inseparable from every such analysis: and if the determination of the quantities of acid employed to saturate these carbonates had been founded on more certain data, the results of his experiments would have been much nearer the truth.

Kirwan's use of the subcarbonates less objectionable.

Berthollet employed the muriatic acid and pure alkalis,

Mr. Berthollet took a more direct method, and the nature of the muriatic acid he employed, being better ascertained, would have led him to accurate results, if the quantity of water, which the muriatic acid gas probably retains, could have been determined; and if he had taken, like Kirwan, the alkaline carbonates as the basis of his labours.

It appears to me, that the preference given to potash and soda prepared with alcohol has introduced into these delicate experiments a source of errour, which is so much the greater as it applies to the substances that predominate in the compounds, the proportions of which were to be ascertained. Mr. Berthollet establishes it as a principle, that 100 parts of potash prepared with alcohol, and kept in fusion for a quarter of an hour, require 61·5 of muriatic acid to neutralize

tralize them; and that 100 parts of soda, prepared in the same manner, take 88. From the results of the experiments above given however, we must infer, that 84·2 of muriatic acid are required, to saturate 100 parts of potash; and 120·5 of the same acid, to neutralize 100 of pure soda: whence it follows, that, the strengths of these alkalis being represented by other numbers, when they are compared with those of bases, the nature of which is fully ascertained, they must give different proportions from those mentioned in the work of Mr. Berthollet.

but the carbonates preferable.

The capacities of saturation of the carbonates, being ascertained by analysis, are liable only to little variation; and then the degree of energy of the muriatic acid approaches nearer to that of the carbonic acid, which has some influence on the results deduced from a comparison of them.

Other neutral salts.

The same reasoning applies to experiments made on the sulphates, nitrates, and phosphates, with potash or soda for their base; but I shall confine myself to a few observations on the experiments, which Mr. Berthollet has published in chap. 18 of the work I have mentioned. To find the quantity of water muriatic acid gas can retain, Mr. Berthollet neutralized 100 parts of potash prepared with alcohol, and kept some time in fusion. The muriate obtained was carefully dried, and weighed only 126·6, instead of 161·5, which ought to have been its weight. Ought not this difference, which is in some measure owing to the water contained in the muriatic acid gas, to be attributed in part to the water, or foreign matter, which forms 0·27 of the potash employed? And may we not thus account for the great differences, that exist between the numbers representing the component parts of muriate of potash in the experiments of Berthollet, Kirwan, and Richter? At least we are naturally led to presume so, from the facts I have given above.

Berthollet's examination of muriatic gas for water defective.

The addition to the alkalis probably water, but this not ascertained.

I regret the not having been able to ascertain the nature of the foreign matter, which is always found combined with soda and potash prepared by means of alcohol. I cannot venture therefore to assert any thing on the subject; but I believe, that water acts a considerable part in these phenomena; and I could have wished to have had time to examine

amine the products, which the two alkalis thus prepared, and exposed to various degrees of heat in contact with different inflammable substances well dried, would have afforded.*

V.

The Bakerian Lecture for 1809. *On some new Electrochemical Researches, on various Objects, particularly the metallic Bodies, from the Alkalis, and Earths, and on some Combinations of Hidrogen.* By HUMPHRY DAVY, *Esq. Sec. R. S. F. R. S. E. M. R. I. A.*

(Continued from vol. XXVI, p. 339.)

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

Queries respecting nitrogen.

ONE of the queries that I advanced, in attempting to reason upon the singular phenomena produced by the action of potassium upon ammonia was, that nitrogen might possibly consist of oxigen and hidrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same path of inquiry, and which at first view do not appear unpromising.

Formation of Nitrogen in various processes erroneous.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter

* Mr. Gay-Lussac, in his report of this paper to the Institute, observed, that Mr. Berthollet, in some experiments which at that time he had communicated only to a few friends, had already found, that potash prepared with alcohol contained at least 0·13 of water, after being exposed to a red heat.

into

into the discussion of the ideas entertained by the German chemists on the origin of nitrogen, produced during the passage of water through redhot tubes, or the speculations of Girtanner, founded on these and other erroneous data; the early discovery of Priestley on the passage of gasses through redhot tubes of earthen ware, the accurate researches of Berthollet, and the experiments of Bouillon Lagrange, have afforded a complete solution of this problem. ously asserted.

Nitrogen supposed to appear in the decomposition and recomposition of water by electricity. Experiment for proof of this

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity*. To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by Voltaic electricity, so as to produce oxigen and hidrogen with great rapidity, and in which these gasses could be detonated, without the exposure of the water to the atmosphere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the Voltaic and common electrical circuit were hermetically inserted into the tube. 500 double plates of the Voltaic combination were used, in such activity that about the eighth of a cubical inch of the mixed gasses, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about half a cubic inch: it had been carefully purged of air by the airpump and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere while warm. After the first detonation of the oxigen and hidrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{10}$ of the volume of the gasses; after every detonation this residuum was found to increase; and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water, i. e. $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas;

* See Dr. Pearson's elaborate experiments on the decomposition of water by electrical explosions. Nicholson's Journal, 4to, vol. I, p. 301.

it

it contained no oxigen; 6 measures mixed with 3 measures of oxigen diminished to 5; so that it consisted of 2·6 of hidrogen, and 3·4 of a gas, having the characters of nitrogen.

apparently in favour of it.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible, that, at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method, by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the preceding one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{3}{100}$ of a cubical inch. It was carefully examined; six measures of it detonated with three measures of oxigen, diminished to rather less than 1 measure. A result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hidrogen. That the hidrogen is in excess may be easily referred to a slight oxidation of the platina.

But the nitrogen probably from the atmosphere.

In the production of water no nitrous acid produced unless nitrogen present.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxigen, hidrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid, sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxigen and hidrogen, and is never produced from those two gasses alone. In the Bakerian lecture for 1806, I have stated several facts, which seemed to show, that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed unless *nitrogen* be present.

Experiments to prove, that no acid or alkali is produced from pure water.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an errour, yet I find, that the assertion, that both acids and alkalis may be produced

duced from pure water, has again been repeated*. The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test, than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two gold cones in hidrogen gas, that no nitrous acid or alkali was formed. It might be said, that in this case the presence of hidrogen dissolved in water would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxigen gas, and the other in an apparatus, in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $\frac{1}{2}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gasses were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases, for two or three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxigen, from oximuriate of potash, was any acid or alkali exhibited in the cones: even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though if the asbestus was touched with unwashed hands, or the smallest particle of neutrosaline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

1st series of experiments.

No acid or alkali appeared,

except when nitrogen is present, or the conductor touched with unwiped hands.

In the second series of experiments, the oxigen and hidrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found, that fixed alkali separated in the glass negatively electrified; and a minute quantity of acid, which

2d. series of experiments.

Alkali in the negative glass, acid in the positive.

* Nicholson's Journal, August, 1809, p. 258.

could

could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities, which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely, that muriatic acid in its arid state may exist in combination in glass.

Muriatic acid in glass.

Platina ignited in oxigen gas and aqueous vapour.

I tried several experiments on the ignition and fusion of platina, by Voltaic electricity, in mixtures of the vapour of water and oxigen gas. I thought it possible, if water could be combined with *more oxigen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxigen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oximuriate of potash, there was not the slightest indication of such a result.

Aqueous vapour passed through redhot oxide of manganese formed nitrous acid uniformly in a large tube.

Water in vapour was passed through oxide of manganese, made redhot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid however was generated: but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

Attempt to produce ammonia from charcoal and pearlash by the action of water.

I stated in the last Bakerian Lecture, that, in attempting to produce ammonia from a mixture of charcoal and pearlash, that had been ignited, by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trial in which the mixture was cooled in contact with hidrogen. I have since made a number of similar experiments. In general when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant, as to be entirely satisfactory; and the same circumstances could

not

not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phenomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner, that the mixture could be cooled in contact with the gas produced during the operation; and that water exhausted of air could be made to act upon the cooled mixture, and afterward distilled from it; figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

Ammonia produced from the same mixture 3 or 4 times,

In all cases, in which the water was brought into contact with the mixture of charcoal and potash when it was perfectly cool, and afterwards distilled from it by a slow heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

and again on the addition of fresh potash.

More ammonia produced, when the mixture is cooled in contact with the atmosphere.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

Perhaps no nitrogen composed in this process.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of Mr. Berthollet, that nitrogen adheres very strongly to charcoal*.

* Mém d'Arcueil. Tom. II, page 485.

The

The circumstances, that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea, that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

Nitrogen produced during the freezing of water.

The experiments of Dr. Priestley upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury while hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing, there was no notable increase of the volume, At the end of the experiment about $\frac{1}{10}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice, and thawed four successive times in a conical vessel of wrought iron. At the end of the fourth process the volume of the gas equalled about $\frac{1}{10}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxigen, $\frac{3}{10}$ hidrogen. and $\frac{6}{10}$ nitrogen.

Nitrous gas and sulphuretted hidrogen kept in contact, diminish much.

Mr. Kirwan observed the fact, that, when nitrous gas and sulphuretted hidrogen are kept in contact for some time, there is a great diminution of volume; and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited, which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results, and I found, that the diminution of the volume of the gasses, when they were mixed in equal proportions, was to rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

Reasonings on this.

In reasoning upon this phenomenon, I saw grounds for a minute investigation of it. Sulphuretted hidrogen, as appears from experiments which I have stated on a former

occasion

occasion, and from some that I shall detail toward the conclusion of this lecture, contains a volume of hidrogen equal to its own. But one of hidrogen demands half its volume of oxigen to convert it into water, and nitrous gas consists of about half a part in volume of oxigen; so that, supposing the whole of the hidrogen employed in absorbing oxigen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed Experiment. together five cubical inches of nitrous gas, and five of sulphuretted hidrogen over mercury, the barometer being at 29·5in., thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gasses quickly diminished; after two days the diminution ceased; and the volume became stationary; the barometer was at 30·45in., and thermometer 52° Fahrenheit; when it equalled 2·3. The gas proved to be about ¼ ni- Results. trous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds: nitrogen, hidrogen, oxigen, and sulphur combining to form one; and a part of the nitrogen and oxigen becoming more condensed, to form another.

Having stated the results of the investigation on the pro- Attempts to duction of nitrous acid and of ammonia, in various pro- decompose ni- cesses of chemistry, I shall notice some attempts that I trogen. made to decompose nitrogen, by agents which I conceived might act at the same time on oxigen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen, without altering it, or being itself changed; but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by

by the intense heat and decomposing energy of Voltaic electricity.

Experiment. I had an apparatus made, by which the Voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phenomena were very brilliant; as soon as the contact with the potassium was made, there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxigen gas.

Results. In all trials of this kind hidrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference, that nitrogen is decompounded in the process; but I found, that, in proportion as the potassium was introduced more free from a *crust of potash*, which would furnish water and consequently hidrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken, the quantity did not equal $\frac{1}{5}$ of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen, which disappeared in any experiment, was $\frac{1}{17}$ of the quantity used; but in this case the crust of potash was considerable, and a volume of hidrogen, nearly equal to $\frac{1}{4}$ of the nitrogen, was produced. It cannot be said, that the nitrogen is *not* decomposed in this operation; but it seems much more likely, that the slight loss is owing to its combination with nascent hidrogen,

gen, and its being separated with the potassium in the form of the gray pyrophoric sublimate, which I have found is always produced, when potassium is electrized and converted into vapour in ammonia.

Experiment with phosphuret of lime.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hidrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

A mixture of nitrogen and oximuriatic acid gas exposed to heat.

As almost all compounds known to contain hidrogen are readily decomposed by oximuriatic acid gas, a mixture of nitrogen and oximuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water; there was a small loss of nitrogen; but the greatest part came over densely clouded, and as nitromuriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

No confirmation of the decomposition of nitrogen.

The general tenour of these inquiries cannot be considered as strengthening in any considerable degree the suspicion, which I formed of the decomposition of nitrogen, by the distillation of the olive coloured substance from potassium and ammonia in tubes of iron.

The loss of nitrogen accounted for.

In reasoning closely upon the phenomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming, that it has been converted into new matter. Though the iron tubes, which I used, were carefully cleaned; yet still it was not unlikely, that a small quantity of oxide might adhere to the welded parts; the oxigen of which, in the beginning of the process of distillation, might form water with hidrogen, given off from the fusible substance; which, being condensed in the upper part of the tube, would be again brought into action toward the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hidrogen. I have made one experiment, with the hopes of deciding this question, in an iron

Experiment to ascertain this.

tube

tube used immediately after the whole internal surface had been cleaned by the borer. Six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hidrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly 1 cubical inch of ammonia, 4 of nitrogen, and 8 of hidrogen. The portion of gas given off in the second operation equalled 3·6 cubical inches; which consisted of 2·5 hidrogen, and 1·1 nitrogen. The potassium lost in the operation was sufficient to generate 3·1 cubical inches of hidrogen.

Part of the potassium united with the iron.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of Mr. Hassenfratz*, that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which I conceive could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2·3 cubical inches of hidrogen were found to be generated.

Apparent loss of nitrogen accounted for.

In the experiments detailed in page 53 of the last volume of the Transactions†, a loss of nitrogen, and a production of hidrogen were perceived in a case, in which the residuum from a portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha*, and it is possible, that ammonia might have been regenerated by hidrogen from the naphtha, and absorbed by that fluid; and a part of the hidrogen might likewise proceed from the decomposition of the naphtha: and in several experiments, in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

* Journal des Mines, April, 1808, p. 275. See Journal, vol. XXV, p. 51.

† Journal, vol. XXIII, p. 252, 253.

Even

Even the considerable excess of hidrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451*, it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me, against the mode of explaining the phenomena by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended. But the question still doubtful.

Though I have already laid before the Society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some farther operations, which have been conducted according to new views of the subject. Farther experiments on the decomposition of ammonia.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxigen was probably separated in this operation; but I was aware, that objections might be made to this mode of accounting for the phenomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxigen in this substance; at the same time it led to the suspicion, that of the two gasses separated by electricity one, or perhaps both, might contain metallic matter united to oxigen: and the results of the distillation of the fusible substance from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity; and I have used an appa- Method of conducting them.

* Journal, vol. XXV, p. 137.

ratus (of which a figure is attached to this paper,) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock, which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

Reason of keeping the volume of gas the same.

The object in keeping the volume the same during the decomposition was to produce the condensation of any aqueous vapour, which, if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gasses,) might, in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hidrogen and nitrogen, as in the ammonia.

Results.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive coloured matter formed on the metal; but though in this case nearly 8 cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to —15°, there was a very feeble indication given of the addition of hygrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, and the hidrogen was to the nitrogen in the average proportions of from 73 or 74 to 27 or 26; the proper corrections being made and the precautions before referred to being taken*.

Assuming

Berthollet's experiments on the decomposition of ammonia.

* Philosophical Transactions, 1809, page 459 [Journal, vol. XXV, p. 143, 144]. Mr. Berthollet, jun., in the second volume of the Memoirs of Arcueil, has given a paper on the decomposition of ammonia, and he enters into an examination of my idea of the oxigen, separated in

Assuming the common estimations of the specific gravity of ammonia, of hidrogen, and nitrogen, the conclusions which I have advanced in the Bakerian lecture for 1807 would be supported by these new experiments, but as the moisture and oxigen visibly separated cannot be conceived to be as much as $\frac{1}{17}$ or $\frac{1}{18}$ of the weight of the ammonia; I resolved to investigate more precisely, than I had reason to think had been hitherto done, the specific gravities of the gasses concerned in their dry state; and the very delicate balance belonging to the Royal Institution placed the means of doing this in my power. Specific gravities of the gasses examined.

Nitrogen, hidrogen, and ammonia, were dried by a long continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be, at 30·5in. barometer, 51° Fahrenheit's thermometer, Sp. grav. of nitrogen, hidrogen, and ammonia.

For nitrogen, the 100 cubical inches....29·8 grains.
For hidrogen, ditto..... 2·27
For ammonia .....18·4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gasses evolved; there is a loss Loss in the decomposition of ammonia.

in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent: and at the same time he confutes some experiments, which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia. His arguments and his facts upon these points appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxigen were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chemist on the direct decomposition of ammonia seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver; which I conceive has occasioned him to overrate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible, that the volume may be exactly doubled, and that the nitrogen may be to the hidrogen as one to three; but, neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

of $\frac{1}{17}$*, and if the smallest proportion be taken the loss will be nearly $\frac{1}{17}$.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

Probably ammonia composed of hidrogen & nitrogen only.

It is *possible*, that the minute quantity of oxigen, which appears to be separated, is not accidental, but a result of the decomposition; and if hidrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hidrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

What is the metallic basis of the volatile alkali?

But if *ammonia* be capable of decomposition into nitrogen and hidrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions, intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

Water always adheres to it.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices, with the hopes of being able

* 100 of ammonia, at the rate of 185, will give 136·9 of hidrogen, weighing 3·1 grains, and 48·1 of nitrogen weighing 14·33 grains; but 18·4 — 17·4 = 1: and at the rate of 180, 133 of hidrogen weighing 30·1, and 47 of nitrogen, weighing 14; and 18·4 — 17 = 1·4.

to

to form it from ammonia in a dry state, but without success. Neither of the amalgams of potassium, sodium, or barium, produces it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases the effect was less distinct, than when pure mercury was used.

By exposing the mercury to a cold of —20° Fahrenheit, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

Driest amalgam obtained.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hidrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from $\frac{4}{10}$ to $\frac{6}{10}$ of hidrogen produced.

Decomposes very slowly.

Attempts to distil ammonium from it.

Ammonia, if an oxide, contains 48 per cent oxigen.

As it does not seem possible to obtain an amalgam in a uniform state as to adhering moisture, it is not easy to say what would be the exact ratio between the hidrogen and ammonia produced, if no more water was present, than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance, in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide,) must contain about 48 per cent of oxigen, which, as will be hereafter seen, will agree with the relations of the attractions

attractions of this alkali for acids, to those of other salifiable bases*.

If hidrogen be a simple substance, nitrogen contains 48 oxigen, 34 basis.

If hidrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly 48 of oxigen, and 34 of basis; but if the opinion be adopted, that hidrogen and nitrogen are both oxides of the same metal, then the quantity of oxigen in nitrogen must be supposed less.

Phlogistic hypothesis.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis of the nature of metallic substances; but, if the facts concerning ammonia were to be reasoned upon, independently of the other general phenomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hidrogen, and metallic by combining with a greater proportion.

Proportions of the amalgam.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter, as it is conceived to contain on the hypothesis of deoxigenation. In the last Bakerian lecture I have rated the proportion at $\frac{1}{11000}$, but this is the least quantity that can be assumed, the mercury being supposed to give off only once and a half its volume of ammonia. If the proportion stated in page 53

* Even in common air, the amalgam evolves hidrogen and ammonia, nearly in these proportions; and in one experiment, which I lately tried, there seemed to be no absorption of oxigen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hidrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that, being in the nascent state, it would rapidly absorb oxigen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxigen had been absorbed, but this difference might have arisen, partly at least, from the mixture of hidrogen. Whether in any case the amalgam absorbs oxigen gas, is a question for farther investigation.

be

be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{1000}$ of new matter, on the antiphlogistic view, and about $\frac{1}{500}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully these ideas, and I shall conclude this section by stating, that, though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese may be applied to the manufacture of nitrous acid. And there is reason to believe, that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

New modes of manufacturing nitrous acid and volatile alkali suggested.

(To be concluded in our next.)

VI.

Times of Migration of some of the Swallow Tribe, &c., near London. In a Letter from THOMAS FORSTER, *Esq.*

To Mr. NICHOLSON.

SIR,

SHOULD you consider the following table, showing the periods of the earliest and latest appearance of several of the swallow tribe, &c., at Clapton, during some years, worth insertion in your Journal, it is much at your service. It may amuse some of your numerous readers, and will oblige your constant reader,

THOMAS FORSTER.

EARLIEST

Time of certain birds appearing and disappearing.

	EARLIEST APPEARANCE.					LATEST.	
	1806	1807	1808	1809	1810	1808	1809
Hirundo rustica, Common swallow.	Apr. 2	May 1	Apr. 18	Apr. 28	Apr. 21	Oct. 17	Oct. 3
Hirundo urbica, Martin.	Apr. 26	May 1	May 1	May 5	Apr. 21	Oct. 18	Oct. 16
Hirundo apus, Swift.		May 16	May 14		May 19	Aug. 14	Aug. 13
Jynx, or yunx torquilla, Wryneck.	May 1	Apr. 30	May 1		Apr. 21		

VII.

The Croonian Lecture. On the Functions of the Heart and Arteries. By THOMAS YOUNG, *M. D. For. Sec. R. S**.

Mechanical motion in a living body subject to the laws of dead matter:

THE mechanical motions, which take place in an animal body, are regulated by the same general laws as the motions of inanimate bodies. Thus the force of gravitation acts precisely in the same manner, and in the same degree, on living as on dead matter; the laws of optics are most accurately observed by all the refractive substances belonging to the eye; and there is no case in which it can be proved, that animated bodies are exempted from any of the affections to which inanimate bodies are liable, except when the powers of life are capable of instituting a process, calculated to overcome those affections by others, which are commensurate to them, and which are of a contrary tendency. For

but the vital powers can institute processes to counteract different affections.

* Philos. Trans. for 1809, p. 1.

example,

example, animal bodies are incapable of being frozen by a considerable degree of cold, because animals have the power of generating heat; but the skin of an animal has no power of generating an acid, or an alkali, to neutralize the action of an alkaline or an acid caustic, and therefore its texture is destroyed by the chemical attraction of such an agent, when it comes into contact with it. As far, therefore, as the functions of animal life depend on the locomotions of the solids or fluids, those functions must be capable of being illustrated by the consideration of the mechanical laws of moving bodies; these laws being fully adequate to the explanation of the connection between the motive powers, which are employed in the system, and the immediate effects, which they are capable of producing, in the solids or fluids of the body: and it is obvious, that the inquiry, in what manner, and in what degree, the circulation of the blood depends on the muscular and elastic powers of the heart and of the arteries, supposing the nature of these powers to be known, must become simply a question belonging to the most refined departments of the theory of hydraulics.

As far as the functions depend on motion they obey mechanical laws,

the circulation therefore an object of hydraulics.

In examining the functions of the heart and arteries, I shall inquire, in the first place, upon the ground of the hydraulic investigations which I have already submitted to the Royal Society*, what would be the nature of the circulation of the blood, if the whole of the veins and arteries were invariable in their dimensions, like tubes of glass or of bone; in the second place, in what manner the pulse would be transmitted from the heart through the arteries, if they were merely elastic tubes; and in the third place, what actions we can with propriety attribute to the muscular coats of the arteries themselves. I shall lastly add some observations on the disturbances of these motions, which may be supposed to occur in different kinds of inflammations and fevers.

Inquiries into the functions of the heart and arteries.

* See Journal, vol. XXII, p. 104. The reader is requested to substitute in p. 121, l. 5 from bot., for $m\sqrt{\frac{a}{1}}$, $m\sqrt{\frac{a}{2}}$; in p. 123, l. 8 and 9 from bot., for when c e d, whence δ; and in p. 122, at the end of l. 6 from bot., to add—is denoted by $a\,v$.

When

The blood vessels considered as tubes of invariable dimensions.

When we consider the blood vessels as tubes of invariable dimensions, we may suppose, in order to determine the velocity of the blood in their different parts, and the resistances opposed to its motion, that this motion is nearly uniform, since the alterations arising from the pulsation of the heart do not materially affect the calculation, especially as they are much less sensible in the smaller vessels than in the larger ones, and the principal part of the resistance arises from these small vessels. We are to consider the blood in the arteries as subjected to a certain pressure, by means of which it is forced into the veins, where the tension is much less considerable; and this pressure, originating from the contractions of the heart and continued by the tension of the arteries, is almost entirely employed in overcoming the friction of the vessels: for the force required to overcome the inertia of the blood is so inconsiderable, that it may, without impropriety, be wholly neglected. We must therefore inquire, what the magnitude of this pressure is, and what degree of resistance we can suppose to arise from the friction of the internal surface of the blood vessels, or from any other causes of retardation. The magnitude of the pressure has been ascertained by Hales's most interesting experiments on a variety of animals, and may thence be estimated with sufficient accuracy for the human body; and for determining the magnitude of the resistance, I shall employ the theorems which I have deduced from my own experiments on very minute tubes, compared with those which had been made by former observers under different circumstances; together with some comparative experiments on the motion of water and of other fluids in the same tubes.

Force with which the blood is propelled from the arteries into the veins.

Dr. Hales infers, from his experiments on quadrupeds of different sizes, that the blood in the human arteries is subjected to a pressure, which is measured by a column of the height of seven feet and a half: in the veins, on the contrary, the pressure appears to amount to about six inches only: so that the force, which urges the blood from the greater arteries through the minuter vessels into the large veins, may be considered as equivalent to the pressure of a column of seven feet.

In

In order to calculate the magnitude of the resistance, it is necessary to determine the dimensions of the arterial system, and the velocity of the blood which flows through it. According to the measurements of Keill and others, we may take $\frac{3}{4}$ of an inch for the usual diameter of the aorta, and suppose each arterial trunk to be divided into two branches, the diameter of each being about $\frac{4}{5}$ of that of the trunk, (or more accurately, $1 : 1{\cdot}26 = 10^{-{\cdot}100567}$), and the joint areas of the sections about a fourth part greater, (or $1{\cdot}2586 : 1 = 10^{{\cdot}099896}$). This division must be continued twenty-nine times, so that the diameter of the thirtieth segment may be only the eleven hundredth part of an inch, that is, nearly large enough to admit two globules of the blood to pass at once. The length of the first segment must be assumed about nine inches, that of the last, the twentieth of an inch only; and supposing the lengths of the intermediate segments to be a series of mean proportionals, each of them must be about one sixth part shorter than the preceding, (or $1 : 1{\cdot}961 = 10^{-{\cdot}07776}$), the mean length of the whole forty-six inches, the capacity to that of the first segment as 72·71 to 1, and consequently the weight of the blood contained in the arterial system about 9·7 pounds. It is probable that this calculation approaches sufficiently near to the truth: for the whole quantity of blood in the body being about 40 pounds, although some have supposed it only 20, others no less than 100, there is reason to believe, that half of this quantity is contained in the veins of the general circulation, and that the other half is divided, nearly in equal proportions, between the pulmonary system and the remaining arteries of the body, so that the arteries of the general circulation may contain about 9 or 10 pounds. Haller allows 50 pounds of circulating fluid, partly serous, and partly red, and supposes $\frac{1}{5}$ of this to be contained in all the arteries taken together: but in a determination which must be in great measure conjectural we cannot expect perfect accuracy: and according to Haller's own account of the proportions of the sections of the arteries and veins, the large trunks of the veins appear to be little more than twice as capacious as those of the arteries, and the smaller branches much more nearly equal, so that

Calculation of the resistance.

Quantity of blood in the system.

that we cannot attribute to the arterial system less than ¼ of the whole blood.

An ounce and half thrown out at each pulsation of the heart.

It may be supposed that the heart throws out, at each pulsation, that is about seventy-five times in a minute, an ounce and a half of blood: hence the mean velocity in the aorta becomes eight inches and a half in a second: and the velocity in each of the succeeding segments must of course be smaller, in proportion as the joint areas of all the corresponding sections are larger than the area of the aorta: for example, in the last order of vessels, of which the diameter is the eleven hundredth of an inch, the velocity will be one ninety-third of an inch: and this result agrees sufficiently well with Hales's observation of the velocity in the capillary arteries of a frog, which was one ninetieth part of an inch only. It is true that Haller is disposed to question the accuracy of this observation, and to attribute a much greater velocity to the blood flowing through the capillary vessels, but he did not attempt either to measure the velocity, or to determine it by calculation: nor is this the only instance in which Haller has been led to reason erroneously, from a want of mathematical knowledge: he may, however, have observed the particles of blood moving in the axis of a vessel with a velocity much exceeding the mean velocity of its whole contents. If we calculate upon these foundations, from the formula which I have already laid before the Society, it will appear, that the resistance which the friction of the arteries would occasion, if water circulated in them instead of blood, with an equal velocity, must amount to a force equivalent to the pressure of a column of fifteen inches and a half: to this we may add about a fourth for the resistance of the capillary veins, and we may estimate the whole friction for water, at twenty inches. The only considerable part of this force is derived from the term $\frac{2{\cdot}1126/v}{10^7 d^{3\cdot5}}$ in the value of f: the term increases for each successive segment in the ratio $1 : 1{\cdot}49425 = 1 : n$, and the sum of the series is to the first term, as $\frac{n^{30}-1}{n-1}$ to 1.

Velocity from 8½ in. to a 93d of an inch in a second.

Haller questioned the accuracy of Hales,

but he sometimes reasoned erroneously,

Resistance from friction if the blood were water.

The resistance very little ex-

It appears also, that a very small portion only of the resistance is created in the larger vessels: thus as far as the twentieth

twentieth division, at the distance of an inch and a quarter only from the extreme capillary arteries, the pressure of a column of one twentieth of an inch only is required for overcoming the whole friction, and at the twenty-fifth division, where the artery does not much exceed the diameter of a human hair, the height to which the water would rise, in a tube fixed laterally into the artery, is only two inches less than in the immediate neighbourhood of the heart. cept in the minute vessels.

In order to judge of the comparative resistance produced by fluids of different degrees of viscidity, I employed the same tubes, by means of which I had determined the friction of water, in extreme cases, for ascertaining the effect of different substances held in solution in the water: since it is impossible to make direct experiments on the blood in its natural state, on account of its tendency to coagulate; and those substances, which have the power of preventing its coagulation, may naturally be supposed to produce a material change in its viscidity. The diameter of one of the tubes, which was cylindrical, was the fortieth part of an inch: the bore of the other was oval, as is usual in the finest tubes made for thermometers: the section, divided by one fourth of the circumference, gave one hundred and seventy seconds for the mean diameter. I caused some milk, and solutions of sugar of different strength, to pass through these tubes: they were all transmitted much more sparingly than water, with an equal pressure, and the difference was more considerable in the smaller than in the larger tube, as might naturally be expected, both from the nature of the resistance, and from the result of Gerstner's experiments on water at different temperatures. In the first tube the resistance to the motion of milk was three times as great as to that of water, a solution of sugar in five times its weight of water produced twice as much resistance as water; in twice its weight, nearly four times as much as water: but in the narrower tube, the weaker solution of sugar exhibited a resistance five times as great as that of water, which is more than twice as much as appeared in the larger tube. Hence there can be no doubt, that the resistance of the internal surface of the arteries to the motion of the blood must be much greater, than would be found in Resistance of fluids more or less viscid. Calculation for blood.

in the case of water: and supposing it about four times as great, instead of 20 inches, we shall have 80, for the measure of a column of which the pressure is capable of forcing the blood, in its natural course, through the smaller arteries and veins, which agrees very well with Hales's estimate.

The calculation founded on preceding observations.

This determination of the probable dimensions of the arterial system, and of the resistances occasioned by its different parts, is in some few respects arbitrary; at the same time that it cannot be materially altered, without altering either the whole quantity of blood contained in the body, the diameters of the smallest capillary vessels, the mean number of bifurcations, or the magnitude of the resistance, all of which are here assumed nearly as they have been laid down by former observers; the estimation of the length of the successive segments only is made in such a manner, as to reconcile these data with each other, by means of the experiments and calculations relating to the friction of fluids in pipes. The effect of curvature in increasing the resistance has been hitherto neglected; it can be sensible only in the larger vessels: and supposing the flexures of these to be equivalent to the circumferences of two circles, each two inches in diameter, the radius q being 1, we have $r = \frac{\cdot 0000045 p v^2 q \frac{1}{2}}{q} = \cdot 0000045 \times 720 \times 64 = \cdot 207$, or about one fifth of an inch, for the additional resistance arising from this cause in the case of water, or four fifths for blood, which is a very inconsiderable part of the whole.

The curvature of the vessel increases the resistance very little.

Objections to the experiments answered.

It might be questioned whether the experiments, which I have made, with tubes $\frac{1}{6}\frac{1}{0}$ of an inch in diameter, are sufficient for determining, with accuracy, the degree in which the resistance would be increased in tubes, of which the diameter is only one sixth part as great; and it may be doubted whether the analogy, derived from these experiments, can be safely employed as a ground for asserting, that so large a portion of the arterial pressure is employed in overcoming the resistance of the very minute arteries. But it must be remembered, that these experiments are at least conclusive with respect to the arteries larger than the tube employed in them, and even those which are a little smaller; so that the remaining pressure, as observed in experiments

periments, can only be employed in overcoming the resistance of the minuter arteries and veins, and these observations tend therefore immediately to confirm the analogy drawn from the experiments on the motion of water. It might indeed be asserted, that the viscidity of the blood exceeds that of water in a much greater ratio than that which is here assigned; but this is rendered improbable by some experiments of Hales, in which, when the intestines were laid open, on the side opposite to the mesentery, so that many of the smaller arteries were divided, the quantity of warm water which passed through them with an equal pressure, was only about twelve times as great as that of the blood which flows through them in their natural state; and it is probable, that at least three or four times as much of any fluid must have passed through them in their divided, as in their entire state, unless we suppose that the coats of the divided vessels, like many other muscular parts, are capable of being contracted by the contact of water. In some other experiments it was found, that a moderate degree of pressure was capable of causing water to exude so copiously through the exhalant vessels of the intestines, that it passed through the aorta with a velocity of about two inches in a second, although these vessels do not naturally allow any passage to the blood: on the other hand, it sometimes happened, that very little water would pass through such channels as naturally transmitted a much larger quantity of blood: a circumstance which Dr. Hales very judiciously attributes to the oozing of the water into the cellular membrane surrounding the vessels, by means of which they were compressed, and their diameters lessened. On the whole, it is not improbable, that in some cases the resistance, opposed to the motion of the blood, may exceed that of water in a ratio somewhat greater than I have assigned; but this must be in the minutest of the vessels, while in the larger arteries the disproportion must be less: so that, however we may view the subject, it appears to be established, that the only considerable resistance, which the blood experiences, occurs in the extreme capillary arteries, of which the diameter scarcely exceeds the hundredth part of an inch.

We cannot suppose, that the dimensions of the sanguife- The truth of

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the inference not affected by the variations that may be supposed in the dimensions.

rous system agree uniformly, in all its parts, with the measures which I have laid down; but the truth of the inference is not affected by these variations. For example, there may perhaps be some arteries communicating with veins, of which the diameter exceeds the eleven hundredths of an inch; but there are certainly many others, which are much more minute; and the blood, or its more liquid parts, passing through these more slowly, it must move more rapidly in the former, so that the resistance may in all be equal to the pressure, and the mean velocity may still remain such as is determined by the quantity of blood passing through the aorta. There is indeed some uncertainty in the measure of the globules of the blood, which I have made the basis of the dimensions of the minute arteries: and I have reason to think, that instead of $\frac{1}{1000}$ of an inch, their greatest diameter does not exceed $\frac{1}{2000}$, or even $\frac{1}{3000}$: the general results of the investigation are not however affected by this difference: it will only require us to suppose the subdivisions somewhat more numerous, and the branches shorter.

Size of the globules of the blood.

These are the principal circumstances, which require to be considered, with respect to the simple transmission of the blood through the arteries into the veins, without regard to the alternate motions of the heart, and to the elastic and muscular powers of the vessels. I shall next examine the nature and velocity of the propagation of the pulse. The successive transmission of the pulsations of the heart, through the length of the arteries, is so analogous to the motion of the waves on the surface of water, or to that of a sound transmitted through the air, that the same calculations will serve for determining the principal affections of all these kinds of motion; and if the water, which is agitated by waves, is supposed to flow at the same time in a continued stream, and the air which conveys a sound to be carried forwards also in the form of a wind, the similitude will be still stronger. The coats of the arteries may perhaps be considered, without much inaccuracy, as perfectly elastic; that is, as producing a force proportional to the degree in which they are extended beyond their natural dimensions; but it is not impossible, that there may be some bodies in nature,

Nature and velocity of the propagation of the pulse.

Elasticity of the coats of the arteries.

nature, which differ materially from this general law, especially where the distension becomes considerable: thus there may be substances, which exhibit a force of tension proportional to the excess of the square, or the cube of their length, beyond a certain given quantity. It is safest therefore to reason upon the elasticity of any substance, from experiments made without any great deviation from the circumstances to which the calculation is to be applied. The law of elasticity may vary.

For this purpose, we may again employ some of the many excellent experiments contained in Hales's hæmastatics. It appears, that, when any small alteration was made in the quantity of blood contained in the arteries of an animal, the height of the column, which measured the pressure, was altered nearly in the same proportion, as far as we are capable of estimating the quantity, which was probably contained in the larger vessels of the animal. Hence it follows, that the velocity of the pulse must be nearly the same as that of an impulse transmitted through an elastic fluid, under the pressure of a column of the same height, as that which measures the actual arterial pressure: that is, equal to that which is acquired by a heavy body falling freely through half this height. In man, this velocity becomes about fifteen feet and a half in a second; to which the progressive motion of the blood itself adds about eight inches; and with this velocity, of at least sixteen feet in a second, it may easily happen, that the pulse may appear to arrive at the most distant parts of the body without the intervention of any very perceptible interval of time. Velocity of the transmission of the pulse.

The velocity of the transmission of the pulse being known, it is easy to determine the degree in which the arteries are dilated during its passage through them. The mean velocity of the blood in the aorta being eight inches and a half in a second, its greatest velocity must be about three times as much, since the contraction of the heart is supposed to occupy only about one third part of the interval between two successive pulsations; and if the velocity of the pulse is sixteen feet in a second, that of the blood itself must be about one eighth part as great; so that the column of blood occupying eight inches may occupy only seven; hence the diameter must increase in the ratio of Degree of dilatation of the arteries.

about fifteen to sixteen. The tension will also become one eighth greater, and the force of the heart must be capable of supporting a column of one hundred and one inches. This force would, however, require to be somewhat increased, from the consideration, that the force required at the end of any canal, during the reflection of a pulsation or wave of any kind, is twice as great as the force exerted during its transmission; and the force employed in the origination of a wave or pulse in a quiescent fluid is the same as is required for its reflection; on the other hand, a weaker pulsation, proceeding into a narrower channel, becomes more energetic, so that, from this consideration, a force somewhat smaller would be required in the heart: on the whole, however, it appears probable, that the former of these corrections must be the more considerable, and that the force of the heart must be measured by the pressure of a column rather more than less than one hundred and one inches high: nor would this force by any means require a strong exertion of muscular power; for it only implies a tension of something less than three pounds for each inch of the circumference of the greatest section of the heart; and supposing the mean thickness half an inch, an equal number of the fibres of some other muscles of the body would be capable of exerting a force of more than two hundred pounds, in the state of the greatest possible action.

The force agrees with an experiment of Hales.

The force, here assigned to each pulsation, agrees extremely well with the inference, that may be drawn from an experiment of Hales, on the ascent of the blood in a tube connected with an artery of a horse. The whole height of the column being nine feet, the blood rose about three inches higher during each pulsation, which was repeated fifty or sixty times in a minute: now we may suppose the acceleration to have extended a little beyond the first half of the space thus described, so that two inches were described in two fifths of a second; and if there had been no friction, nor any other cause of retardation, there can be no doubt, that at least four inches would have been described in the same time; but the same column of nine feet, if it had been actuated by its own weight, would have described thirty one inches in the same time: consequently the force, with

with which the blood was forced through the artery, was nearly one eighth of the whole force of tension, as it appears in the former calculation.

Diminution of the pulse in the smaller arteries.

The magnitude of the pulse must diminish in the smaller arteries in the subduplicate proportion of the increase of the joint areas, in the same manner as the intensity of sound is shown to decrease in diverging from a centre, in the subduplicate ratio of the quantity of matter affected by its motion at the same time. For example, in the arteries of the tenth order, of which the diameter is one thirteenth of an inch, its magnitude must be only one third as great as in the aorta, that is, the greatest progressive velocity of the blood must be eight inches and a half in a second only, and the dilatation one fiftieth part only of the diameter. In the vessels of the twentieth order, the dilatation does not exceed $\frac{1}{1000}$ of the diameter, which is itself the 140th part only of an inch: so that it is not surprising, that Haller should have been unable to discover any dilatation in vessels of these dimensions, even with the assistance of a powerful microscope. If we estimated the magnitude of the pulse in the aorta, from the excess of the temporary above the mean velocity, which would perhaps be justifiable, that magnitude would be still less considerable.

Velocity of the pulse in the larger arteries more considerable than here stated.

These calculations agree extremely well with each other, and with experiment, as far as they relate to the power of the heart, and the affections of the smaller arteries. But there is reason to think, that the velocity of the pulse in the larger vessels is much more considerable, than has been here stated; and their dilatation is also less conspicuous, when they are exposed to view, than it would probably be, if it were as great as is inferred from the velocity here assigned. I have demonstrated in the hydraulic investigations which I lately laid before the Royal Society, that the velocity of an impulse passing through a tube, consisting of perfectly elastic materials, is half as great as that of a body supposed to have fallen from the given point to the base of the modular column of the tube; and that the height of this column is such, that the tube would be extended without limit by its pressure: consequently it must be greater than the height of a column equivalent to the pressure

Pressure required to burst the carotids of a dog.

by which the tube is burst. Now it has been ascertained by Dr. Hales, that the pressure, required for bursting one of the carotids of a dog, is equal to that of a column of water one hundred and ninety feet high; nor does he remark, that the artery was very materially dilated; and deducting from this height the five feet, which express the actual pressure in the arteries of a dog, the remaining one hundred and eighty five feet will give a velocity of at least fifty four feet in a second, for the propagation of the pulse in the dog. It is not however ascertained, that all the membranes, which may have surrounded the artery in this experiment, are called into action in its ordinary pulsation; much less that the force, developed by their tension, varies precisely according to the general law of perfectly elastic bodies; but this mode of calculation is still amply sufficient to make it probable, that the velocity of the pulsations, in the larger arteries, must amount to at least forty feet in a second, although some very considerable deductions must be made, on account of the resistances of various kinds, which cannot be comprehended in the calculation.

The artery does not subside to its former dimensions immediately.

The artery must not be supposed to subside, immediately after each pulsation, precisely to its original dimensions; since it must remain somewhat fuller, in order to supply the capillary arteries, and the veins, in the interval between the two successive pulsations: and in this respect it differs from the motions of a wave through a canal, which is open on both sides: but the difference may be understood, by supposing a partial reflection of the pulse to take place at every point where it meets with any resistance, which will leave a general distension of the artery, without any appearance of a retrogade pulsation.

(To be concluded in our next.)

VIII.

Letters from Dr. WILLIAM ROXBURGH, *of Calcutta, to Dr.* C. TAYLOR, *Secretary to the Society of Arts, &c., on various Natural Productions of the East Indies*.*

MY DEAR SIR,

IT will give you pleasure to learn, that I and my family arrived at Bengal in very good health. I have not had much time to prepare any kind of communication for the Society, but shall not be idle. I trust that I shall by early conveyance receive your letter from Dr. Hunter, the Secretary of our Asiatic Society, to accompany the copy of the Transactions of the Society of Arts. Tell me what is thought of the extract of the gaub, or tannin, I sent you prepared from the fruit of diospyros glutinosus, or rather embryopteris glutinifera, *Coromandel Plants, Vol. I, No.* 70; you know you were only just put into a way of getting it from the India house, when I left you.

Extract of gaub or tannin.

I propose to get Mr. Cowper, the surgeon of the ship we came out in, to carry this, and a sample of the fibres of No. 3 of my last communication, on the Comparative Strength &c. of the Plant called Calooee by the Malays, see Vol. 24, page 148†. I can cultivate this plant to any extent, as it grows readily from slips and cuttings, is perennial, and yields three or four cuttings, or crops, annually; but the cleaning of the fibres from the glutinous fleshy exterior coat, with which they are covered and intermixed, is uncommonly difficult. It has been simply scraped off in the sample I now send you, which I think you may present to the Society, though I fear this letter is written in too great a hurry for their attention. I beg of you to try to procure me all the information you can relative to cleaning such fibres. When the shoots are cut, the bark peels off most readily, but no

Fibres of a shrubby species of nettle.

* Trans. of the Society of Arts, vols. XXVI and XXVII.

† See Journal, vol. XVI, p. 226, 228.

kind

kind of washing, coction, or maceration, that I have yet been able to think of, is of any use in cleaning or freeing the fibres from the exterior coat; the best way I have yet tried is scraping off this coat, as they do the pulpy part of the wild plantain, or abaca at Manilla; see Annals of Botany, vol. 1, p. 200; but such a process will, I fear, be too expensive for calooee hemp, though I know it is much stronger than any thing of the kind I could ever procure from the plantain tree indeed next to *jeetee;* this fibre is the strongest vegetable fibre known to me.

Hemp from it. I have put up two small samples of the calooee hemp. No. 1 is prepared as before mentioned, by scraping off the exterior coat as soon as the bark is pulled off. This has been cut and cleaned within these two weeks. No. 2 is the bark peeled off and dried in that state, and is about one year old, consequently done while I was in England. No. 1 seems to me to be as clean as the generality of Russian hemp. Pray let Lord Dundonald see this substance, and make my best respects to his lordship when you see him; he may be able to advise me how to proceed in cleaning it in the first instance.

Orange dye. Remember me to Dr. Bancroft, and tell him I have not forgot the orange dye, wassuntagonda, a powder procured from the outside of the capsules of my rottleria tinctoria; I must procure it from a distant country.

Gum kuteera. I have been this instant looking over the twenty-first Volume of your Society's Transactions, and think it may be agreeable to you to know, that the tree which yields the *gum kuteera*, page 423, is my STERCULIA URENS. *(See Coromandel Plants, Vol. I, No. 24.)*

I am, &c.

W. ROXBURGH.

Calcutta, Sept. 20, 1807.

My Dear Sir,

Since I wrote to you, on the 20th of September, by the surgeon of the Baring, who carried for you samples of the Malay

Malay hemp, called calooee, I have received your letter of the 7th of March, intended to overtake me at Portsmouth, and I thank you for the pains you have taken about my myrobalan galls. If the value of them is, by this experiment, ascertained, I shall the less regret the great loss I have sustained by them. You will be able to learn this from Mr. Desanges, and let me know. Myrobalan galls.

You have now learned how to get a treasury order for any thing I may send the Society, I shall therefore be encouraged to trouble you oftener, and just now with four pounds and a half of the extract of gaub fruit, (EMBRYOPTERIS GLUTINIFERA, *Coromandel Plants, Vol. I, No. 70)*, which is at this instant in perfection, and the extract is made with cold water. The former, which by the above mentioned letter I learn you were about to receive, was made with hot water. The fruit to make this quantity of extract, four pounds and a half, cost sixpence, and the expense of making may be as much; this information will the better enable the Society to ascertain whether or not it can be useful to tanners or others in England. The rate of freight you can better determine than I can here. Extract of the gaub fruit.

The little box is not quite full with the extract. I have filled it with calooee hemp, the produce of the second cutting of the same plants in two months, so I may safely conclude four crops or cuttings may be had annually. Calooee hemp.

I am, &c.

W. ROXBURGH.

Calcutta, Nov. 3, 1807.

My dear Sir,

To convince you, that I have not forgot the Society nor you, I send you, above, copies of two letters which I have written to you since my return to this place. I also enclose a letter from the Secretary of the Asiatic Society, to convince you that I have been a faithful agent for establishing connections between the two Societies.

If you value our labours as we do ourselves, the Calcutta price of each volume being fifty rupees, or

crowns, you will also see we have not been insensible of the attentions of the Society of Arts.

Sun hemp. I am not relaxing in my pursuit after substitutes for hemp and flax; some more experiments are beginning, and are very far advanced, which promise success, that is, cultivating our sun or Indian hemp, during the dry season, as practised at or near Bombay, and at Malabar, where their sun or hemp has been reckoned in London equal to, if not better, than the best Russia hemp.

East India canvas. Canvas is now made here in very large quantities by two or three clever Europeans, from the common sun plant of this country, of so very good quality, as to have nearly superseded English canvas throughout India. This is gaining a great point, if England should ever be pushed for the raw material, as the freight of canvas from hence to Europe will be trifling when compared to the freight of the raw article.

Lignum vitæ. I was told in England that lignum vitæ was becoming scarce and dear. I am inquiring after a substitute, but hitherto without any pointed success. Should I meet with any kind of wood, that promises well, I will send the Society a specimen, and another specimen of a kind of very beautiful elegantly veined black wood for furniture, called here *seet-saul.*

Elegant black veined wood.

I remain, yours, very sincerely,

W. ROXBURGH.

Botanic Garden, near Calcutta,
Feb 9, 1809.

My dear Sir,

Resin of valeria indica. Since my last, of the 9th of February, this year, I have got some farther matters to communicate. In the first place, this will be accompanied by a sample of the resin of the large Malabar tree, called by botanists *valeria indica**. It appears to me to resemble amber more than copal. It may perhaps be very pure copal, and in this state, like amber

Electric by friction.

* No. 1564 of my drawings of Indian Plants, sent to the Court of Directors.

when

when rubbed on paper, as I have this moment tried it, exhibits electric powers, by attracting small bits of paper, feathers, &c. I however do not mean to point out its qualities, but rather send this sample for information, requesting you to get it examined by some well-qualified persons, and let me know the result. Large quantities may be had in this country to send to Europe, if it is found useful, and will answer in price.

A similar resin sold for amber.

There is brought annually from Muscat in Arabia to this market considerable quantities of a similar resin, under the Persian name *kahroba*, which signifies *amber;* some of this I have also the pleasure to send you, and also beg to be informed of its nature and qualities. The purest pieces are susceptible of a fine polish, and are here cut into beads and ornaments, which are much worn by the natives as well as European ladies. I once saw a very beautiful string of these beads sent to England under the name of amber beads. The most beautiful amber-coloured pieces are therefore the most valuable, and are sold for about a shilling the pound by retail in the bazar. The less pure pieces and the green-coloured are at a much lower rate. My correspondent, who resides where the tree grows in the elevated lands of Malabar, sent me chiefly green pieces, thinking, no doubt, they were the most beautiful, and would therefore be the most acceptable.

Olibanum.

In the 9th volume of the Researches of the Asiatic Society at Calcutta is a paper on olibanum, by Mr. Colebrook, the president; some of this article, which he was so good as to give me to send to the Society, will accompany this letter; we both wish you would get it examined, and favour us with an account thereof as early as possible, particularly if sending it to London for sale can answer any good purpose. I have not yet got any thing which I think will answer for lignum vitæ, nor have I yet got the log of black wood (seet-saul, Hind.) mentioned in my last letter. In the same parcel with your three specimens above-mentioned, I have put one of the resin of *valeria indica*, and one of olibanum, for the Company's Museum, which I request you to send with my compliments

but too much moisture is more dangerous than drought. When the weather is dry, it will be necessary to give them a little fresh water now and then, the periods and quantity cannot be pointed out in any instructions, as the state of the weather must be the guide.

Directions where to place the chests cannot well be given, as that will in a great measure depend on the size, structure, &c. of the ship. In our Indiamen, round the capstan on the quarter-deck seems the best on many accounts. The greatest danger in such a situation is while the deck is washing in the mornings, the boxes must then be shut, and covered with a piece of canvas, or something to prevent the salt water getting in between crevices.

When plants from a cold climate get into a warm one, they shoot most luxuriantly, and often kill or choke one another; the longer shoots must therefore be frequently shortened, and as many of the leaves thinned as will give the rest air and room. Insects, particularly caterpillars, often make their appearance about the same time, they must be carefully picked off.

Roots and succulent plants. Baskets with roots, (such as potatoes, &c.) or succulent plants, may be hung up in any cool, airy place, such, for example, as the projecting part of the deck which covers the wheel in our Indiamen, or hung over the stern, but in that case they must be covered with a tarpaulin or painted canvas.

Seeds. Seeds ought to be kept in a cool, dry place, and never put below in the gun-room, hold, or lower deck.

Roots. Roots ought to be packed in dry sand, after being moderately dried, and despatched in any ship that sails about the close of the year.

IX.

Cultivation of Poppies with Carrots[*].

Carrots advantageously IN some parts of Germany poppy and carrot seeds are sown together. On light soils the poppy branches out but

[*] Sonnini's Biblioth. Physico-économ. Oct. 1808, p. 291.

little,

little, and its roots are scarcely sheltered from the strong heats. The carrot covers these roots with its leaves, and preserves them from drought, by retaining the moisture in the ground: at the same time it allows the poppy to enjoy the sun and air freely; and cannot injure it in the ground, as its root strikes perpendicularly downward, while that of the poppy ramifies near the surface. In this way the produce of the ground is doubled. An experiment shows, that the poppy is not injured either in the quality or quantity of its produce by this practice. cultivated with poppies.

Carrot seed was sown in the intervals between the poppies on a quarter of an acre of land. The harvest produced 3 simmers [near 7 bushels] of poppy seed, from which were expressed 12 quarts of clear and well flavoured oil, and 21 pints of thicker oil. The former, at the current price of 36 krentz. [1*s.* 5*d.*] a quart, and the oil cakes, at 3 kr. [near 1½*d.*] apiece, fetched 40 fl. 42 kr. [£4 15*s.*]. This is exclusive of the thick oil, the carrots, and the tops of the carrots eaten by the cattle as fodder. Calculation of produce.

X.

*Method of preserving and keeping in Vigour Fruit Trees planted in Orchards or Fields**.

IT has been observed, that the numerous roots of the herbage growing round fruit trees, recently planted in fields and orchards, are injurious to the vegetation of these young trees; and their fruit is smaller and inferior in quality, in proportion to the quantity of the herbage that covers their roots. This is particularly the case with peach trees. In Germany, to prevent this, they surround the fresh transplanted trees with the refuse stalks of flax, after the fibrous part has been taken off, spreading it over the ground as far as their roots extend; and this gives them surprising vigour. No weeds will grow under this flax, and the earth remains fresh and loose. Herbage injurious to the roots of young fruit trees. prevented by covering them with flax stalks.

This experiment has been tried on an old peach tree, lan- This revives

* Sonnini's Biblioth. Physico-économ. Sept. 1808, p. 161.

guishing

the vigour of old trees.

guishing in an orchard. Refuse flax stalks were spread at its foot, and far enough round to cover all its roots; when it soon recovered its strength, pushed out vigorous shoots, and was loaded with larger and better fruit than before.

Dead leaves answer the same purpose.

The leaves of trees falling in autumn may be employed in the same way with advantage; but dry branches, or something else, should be laid over them, to prevent their being blown away by the wind. The leaves of walnut trees appear to produce the best effect.

SCIENTIFIC NEWS.

Recent discoveries.

THE substance of the late discoveries communicated by Professor Davy to the Royal Society is as follows.

Oximuriatic acid

1. That the oximuriatic acid is a simple body, belonging to a class, in which two bodies only at present are known, this and oxigen.

analogous to oxigen.

2. That like oxigen it forms bodies, which are either acids, or analogous to acids, or oxides, by combining with combustible bodies.

Muriatic acid.

3. That hidrogen is the basis of the muriatic acid, and that oximuriatic acid is its acidifying principle.

New compounds.

4. That phosphorus, sulphur, tin, arsenic, &c., by combining with oximuriatic acid, form substances analogous to acids, which have the power of neutralizing ammonia, and probably other alkalis, and of forming combinations with other compounds of the same class.

One analogous to an earth.

5. That phosphorus acidified by oximuriatic acid forms a compound with ammonia not decomposable by a white heat, and having characters analogous to an earth.

Compounds of oximuriatic acid.

The combinations of oximuriatic acid with inflammable bodies offer objects of investigation of a perfectly novel kind, analogous to, and scarcely less interesting than those belonging to the combinations of oxigen.

More than one acidifying principle.

The chemists of the phlogistic school supposed only one principle of inflammability. Lavoisier, in his beautiful generalization, was acquainted with only one acidifying principle, or one principle which rendered bodies soluble: but there is actually another known, viz. oximuriatic acid; and it is not impossible, but others may be discovered.

London

London Hospital.

Dr. Buxton's lectures on the practice of medicine, will be commenced on Monday, the 1st of October. Medical lectures.

St. Thomas's and Guy's Hospitals.

The autumnal courses of lectures at these adjoining hospitals, will begin the first week in October: viz.

At St. Thomas's.

Anatomy and the operations of surgery, by Mr. Cline and Mr. Cooper.—The principles and practice of surgery, by Mr. Cooper.

At Guy's.

Practice of medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental philosophy, by Mr. Allen.—Theory of medicine, and materia medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and diseases of women and children, by Dr. Haighton.—Physiology, or laws of the animal economy, by Dr. Haighton.—Structure and diseases of the teeth, by Mr. Fox.

N. B. These several lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a *complete course of medical and chirurgical instructions.*

To Correspondents.

My correspondent R does not seem to be aware, that the heights of the barometer are not taken at the same time of the day by Mr. Gilpin and Mr. Bancks.

It does not appear when Zahn's experiment on the radiation of cold was made, from the account of Prof. Kries, who only mentions it incidentally; and I have not his works to refer to. I do not think it necessary therefore, to copy from Musschenbroeck the account of a similar experiment made by the Academy del Cimento, and published in their transactions for 1667.

Mr. Knight's paper is necessarily deferred on account of the plate.

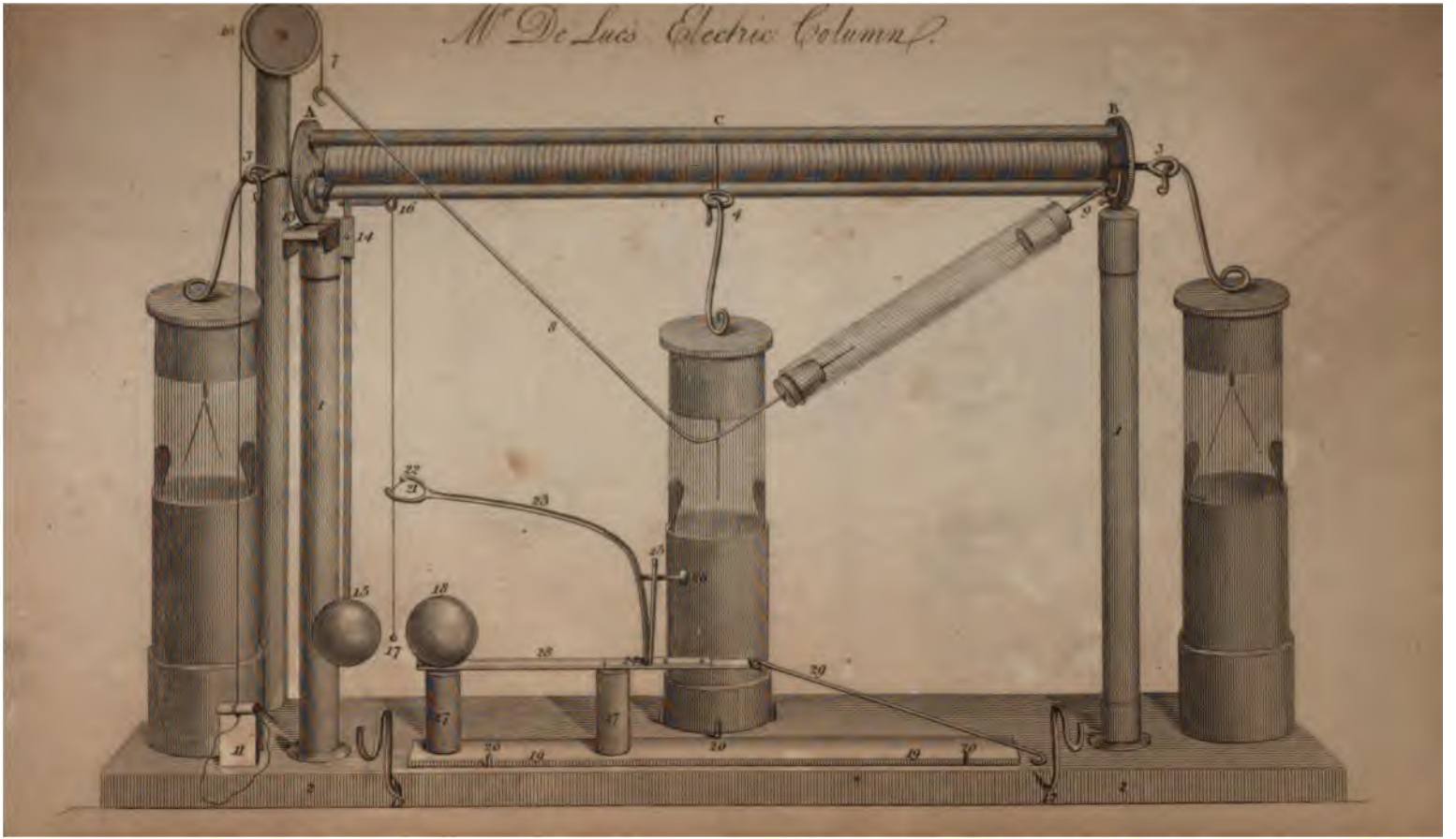
METEOROLOGICAL JOURNAL,

For AUGUST, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JULY Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
27	60°	57°	62°	52°	29·63	Rain	Cloudy
28	60	55·5	61	51	29·58	Ditto	Fair *
29	58	59	63	56	29·93	Ditto	Cloudy
30	62·5	60	64	52·5	29·71	Ditto	Ditto
31	60	58·5	65	49	29·83	Ditto	Fair
AUG.							
1	57	59·5	62·5	52	29·82	Ditto †	Ditto
2	59	62	66	57·5	29·98	Fair	Cloudy
3	62	63	69	55	29·87	Rain	Fair ‡
4	61	60·5	66	53·5	29·62	Ditto	Ditto
5	59	57	63·5	49·5	29·64	Ditto §	Ditto
6	57·5	61	66	56·5	29·77	Fair	Rain
7	62	61·5	65	57	29·65	Rain	Cloudy
8	61	60	65	52	29·57	Ditto	Ditto
9	58	59·5	62	57	30·00	Fair	Ditto
10	62·5	62	69·5	52	29·79	Ditto	Ditto
11	60	59	66·5	53	29·64	Rain	Ditto
12	60	62	67	55	29·87	Ditto	Cloudy
13	59	61	65	56	29·54	Fair ‖	Ditto
14	62	61	64	54·5	29·83	Cloudy	Ditto
15	57	57	63·5	48·5	29·55	Rain	Ditto
16	52	52	54	47	29·52	Ditto	Ditto
17	53·5	55	60·5	45·5	29·87	Fair	Fair
18	55	59·5	66·5	53·5	30·20	Ditto	Ditto
19	59	58	63	49	30·18	Rain	Fair
20	58	64·5	69	51	30·28	Fair	Ditto
21	59	64	70·5	50·5	30·24	Ditto	Ditto
22	59	64·5	72	51	30·16	Ditto	Ditto
23	62·5	67·5	73	58	30·02	Ditto	Ditto
24	64·5	68·5	75·5	59	30·06	Ditto	Ditto
25	64	68·5	75	59	30·02	Ditto	Ditto ¶
26	62·5	67	70	57	29·97	Cloudy **	Ditto

* Rain, A. M. and P. M. Cold evening.
† Thunder at 5 P. M. ‡ Rain at 11 P. M.
§ Tremendous thunder, vivid lightning, and heavy rain, about 1 P. M.
‖ Boisterous morning. ¶ Cloudy at 11 P. M. with cool breeze.
** The morning only.


M^r De Luc's Electric Column.
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PUBLIC
ASTOR
TILDEN

Mr. Shutes Cupping Instrument.

Fig. 1.

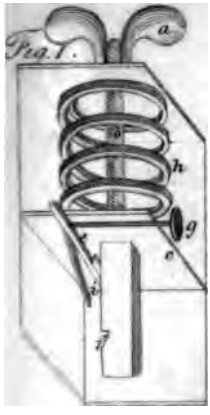


Fig. 2.

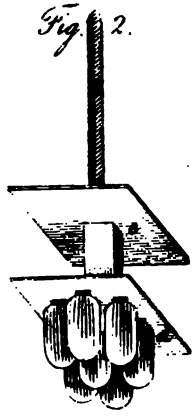
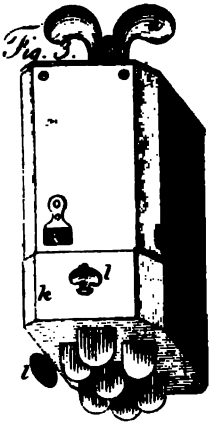


Fig. 3.



Mr. Knight on Capillary Attraction.

Fig. 4.

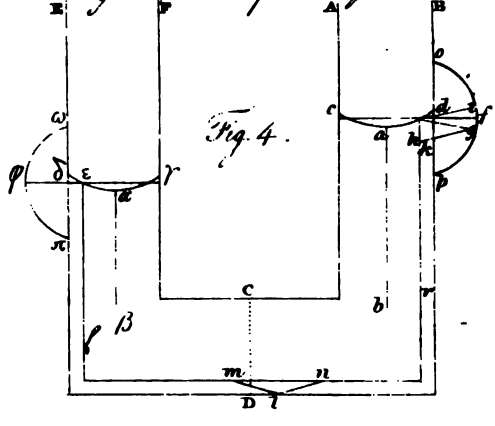


Fig. 6.

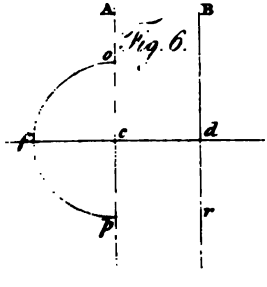
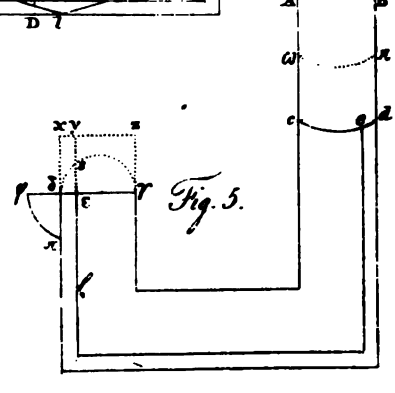
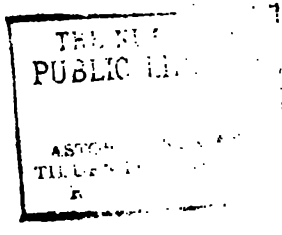


Fig. 5.




PUBLIC

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1810.

ARTICLE I.

On the Electric Column and Aerial Electroscope. By J. A. De Luc, *Esq. F. R. S.*

THE principal result of my paper *on the Analysis of the Galvanic Pile* has been to show, that by this instrument, in which Sig. Volta has so much extended Sig. Galvani's first discovery of some physiological effects produced by two associated metals, we have been really enabled to determine whence proceeded that action upon the animal economy. When these effects were discovered by Sig. Galvani, appearing similar to the *shock* produced by the Leyden vial, they pointed out some action of the *electric fluid*: but when this *fluid* acts thus upon our organs, it is also manifested by *electric motions* and by *sparks*; whereas not even the first of these signs appeared in the galvanic experiments. Therefore the action of the *electric fluid* in these first phenomena might for ever have remained doubtful, had not Sig. Volta, by the invention of his admirable *pile*, increased that action, so as not only to be attended with *electric motions* and *sparks*, but to produce some *chemical effects* known before to belong to the *electric fluid*.

Galvanism shown to be a modification of electricity by Volta's pile.

Two distinct effects shown in the pile.

But there remained a great question. These different effects had before been produced by the *electric fluid* only when it was arrived at a great *density*; while the *pile* produces the same effects with so small a quantity of the *fluid*, as to be often hardly sufficient to move the *gold leaf* electroscope. This has been the object of the experiments contained in my first *paper*, which have manifested two distinct effects in the *pile*: 1. A *motion* of the *electric fluid* produced by the association of two proper *metals*, independent of any other effect: 2. A *modification* of this small quantity of *electric fluid*, on pervading the *pile* during the *calcination* of some of its *metals*. A distinction first shown by different *dissections* of the *pile*, and afterward directly proved, by producing an instrument, which retains the *electric effects* of the *pile* by the association of two *metals*, without either its *chemical* effects, or the *shock*.

A new instrument,

the electric column,

This new instrument was to be distinguished from the galvanic (or voltaic) pile; therefore, in my second *paper* delivered to the Royal Society on this subject, I named it *electric column*, as being a spontaneous and permanent *electric machine;* and as it manifested also, by changes in its *electroscopes*, some variable influence of the *electric state* of the *air*, this new effect was to be expressed by an additional name. It is different from the indications of our former *atmospheric electroscopes*, such as elevated *conductors* and *kites*, which inform us only of the *comparative* states of the *stratum* of *air* that they attain, and of the *air* at the place of observation; without any indication of the *changes* in the latter, probab'y connected with some phenomena, which we observe, without knowing their cause; and as the *electric column* seemed to promise a method of discovering these *changes*, I named it also *aerial electroscope*.

manifesting some variable influence of the electric state of the air.

Such had been the principal object of my second *paper* presented to the Royal Society; it was only in its nascent state; but as I thought it worthy to be taken up and followed by other experimental philosophers, I would not postpone communicating it to the public till it was more advanced: it has not been published in the Phil. Trans., and as I have since carried it farther, I shall here treat it in a different manner, dividing it into three parts: the *first* will

Object of the present paper.

will relate to the *electric column*, considered only as the *electric efficiency* in the *pile*, divested of either *chemical* or *physiological* effects. The *second* will explain the difficulties which I have encountered in attempting to bring the instrument to its desirable function as *aerial electroscope*, and the point which I have attained in this new kind of *meteorological* observations. And in the *third*, I shall offer to the attention of the natural philosopher, some *meteorological* observations, which show the importance, in every branch of experimental philosophy, and especially in chemical theories, of forwarding the observation of atmospheric phenomena.

Part I.

On the Electric Column.

I have explained in the first *paper* my system concerning the cause of a *motion* of the *electric fluid*, produced by the properties of two associated *metals*; and as all the circumstances attending this *motion* are characteristic of its cause, this connexion will be here my principal object. Cause of the effects of the pile.

I shall begin by some experiments under the same form as Exp. 4 in the former paper, in which the *brass tripods* were placed between the two *metals*, and these *groups* separated by pieces of *wet cloth*. In this first dissection of the *pile*, both *electric* and *chemical* effects were produced, the latter on account of the *water* contained in the cloth; and I wanted to know what would be the consequence, in the same arrangement, with respect to the *electric* effects of suppressing the *chemical* ones, by a *dry* intermediary substance. For this purpose, I substituted for the *silver* plates and the *wet cloth* of the former experiment pieces of *Dutch gilt paper*, placing the *brass tripods* between the *zinc* plates and the *copper* side of the papers; and after having found, that 40 such groups could be contained in each column of the frame described in that paper, forming in the whole 80 groups, I made the following experiments. Electric and chemical effects. The chemical suppressed.

Exp. 1. In order to judge what would be the effect of interposing the *brass tripods* between the *metals*, I first mounted the 80 groups without them, and observed the de- Exp. 1.

gree of divergence of the *gold leaves* at both extremities. *Dry* paper not being so good a *conductor* as *wet* cloth, the *electric* effects were not so great as they would have been with the latter; but they were sufficient for my purpose. I kept this apparatus for some days, observing the divergences in different parts of the day, and the greatest, which happened to be at the *zinc* side, was of 0·3 of an inch.

Exp. 2. *Exp.* 2. I then placed the *brass tripods* between each *zinc* plate and the *copper* side of the papers, the *paper* side of which separated the *binary* groups of *metals*, as did the *wet cloth* in the former experiment: the *electric effects* remained the same as they were in *Exp.* 1, without the *tripods*.

It came then into my mind, that the *tripods*, being of *brass*, might alone produce some effect, with only plain *paper* to separate these groups of *zinc* and *brass*; but the latter having but a small mass, the transmission of *electric fluid* along the *column* could not but be *slower*; and this was the very reason which determined me to the trial, as an experiment that would also relate to the cause which renders the *column* an *aerial electroscope*: this cause is the action of the *ambient air*, the immediate effects of which are, to lessen the *positive* state of one of the extremities, and the *negative* state of the other, according to its own *electric* state; and more *slowness* in the *motion* of the fluid giving more *time* to this action of the *ambient air* for diminishing the *electric* indications at the extremities of the *column*, these symptoms were to be smaller. This therefore induced me to make the following experiment.

Effect of the air.

Exp. 3. *Exp.* 3. I mounted the column with 80 groups composed of *zinc* plates with only the *brass tripods*, separating them with pieces of *writing paper*, and I kept also this column for some days, observing the *electroscopes* at its extremities: they had the same variations which I had before observed, but very small, and the greatest divergence, which, as it is commonly, was in the middle of the day, did not exceed 0·1 of an inch.

Slowness of the motion of

I now come to that *slowness* mentioned above in the *motion* of the *electric fluid* produced by the property of the *column*,

column, which being attributed to a *fluid* known to possess, in the proper sense, the swiftness of *lightning*, must appear a paradox. Rapidity of motion certainly belongs to the *electric fluid*, when darting in a torrent; but the *electric matter*, of which it is composed, has a tendency to adhere to all bodies, *air* included; and it is this very property, as explained by Sig. Volta, which occasions the *electric motions*, when the *fluid*, tending to move by a rupture of its *equilibrium*, is more reluctant to be separated from the body which possesses it, than the latter to follow the *fluid* in its motion.

the fluid in the column,

from its tendency to adhere to bodies.

The effect of this tendency of the *electric fluid* towards bodies, in retarding its *motion* within the *column*, is analogous to an effect observed in water. When *water* is kept in motion in a channel by a constant supply, it is seen to take its course in the middle, leaving behind the particles retarded by their tendency towards the sides, which is decreasing as the distance increases. But the analogy is more direct, when *water* is confined in a pond, beset with aquatic plants or other impediments; for the *motions* impressed on that *water* at one side of the pond, though continued, if small, are but *slowly* and seldom *completely* communicated through the whole space. The case is the same with respect to the *electric fluid* set in *motion* by the property of the *column*, not only when confined within it, but when a *current* is produced; which effect will be shown by some experiments, after I shall have explained some parts of the figure annexed to this paper. See Plate III.

Analogy in water.

The dimensions of this *figure* are half those of the original: it consists of many parts which I shall successively describe, beginning by those which relate to my present object: its fundamental parts are an *electric column*, with its *electroscopes*. The former is represented at A, B, supported horizontally on two pillars 1, 1, consisting of solid glass rods, covered with *sealing wax*, or with some other insulating varnish, and fastened on the wooden base 2, 2, by female screws underneath. The *column* itself is composed of 600 groups, formed of *zinc* plates 0·7 inch diameter, and equal pieces of plain *Dutch gilt paper*; the *copper* side of which being turned towards A, this is the *positive* extremity of

Explanation of the plate.

of the *column*: and as also, in every group, the *paper* itself serves only to separate the *binary* groups of *zinc* and *copper*, the latter being in each of them on the side of B, this is the *negative* extremity. The groups are contained between three glass rods, covered with sealing wax melted over them while hot, and fixed in holes of the brass plates A, B, where they have been introduced while the plates were hot, and the holes filled with sealing wax. These brass plates have in their lower part a pin, which enters freely into the brass cap at the top of the pillars 1, 1. At the extremities of the *column* are screws 3, 3, formed on the outside in the shape of loops: they serve first, to press the groups between the glass rods; and besides to produce, by brass wires hooked in their loops, the communication of each extremity with the nearest *electroscope*, as represented in the *figure*.

This column too powerful for some experiments.

In general this *column* produces too great effects for the experiments which I have here in view, as the *gold leaves* strike the sides of the electroscopes; while there should be merely a simple *divergence*: therefore, either a smaller *column* must be used, or the time must be chosen when the 600 groups produce only this effect.

Exp. 4.

Exp. 4. Having observed the actual quantity of *divergence* in both electroscopes, when I lay my finger on the top of either of them, in order to produce the communication of its extremity of the *column* with the ground, the *divergence* ceases in it, and becomes nearly double in the other. Then taking off my finger, and thus abandoning the *column* to its own operation, the divergences are not immediately restored to their former quantity; it requires some time to produce them again, even half an hour or an hour.

Slow motion of the fluid does not take place in all cases.

This shows a *reluctance* in the parts once possessed of the *electric fluid*, to obey the cause which requires more of it on *zinc* than on *copper*, in order to establish the *electric equilibrium* between them. But this concerns only the quantity of *electric fluid*, which the *column* possesses in common with the surrounding bodies and the ambient air; for at the same time that this quantity delays to obey the law of the *column*, if an insulated body, either *positive* or *negative*,

negative, but in a degree which can merely affect the gold leaf electroscope, be applied to one extremity, the effect is instantly perceived at the other. This again is the same as if a stream of water were introduced at one side of the *pond* of the above example, and an opening made at the opposite side; for a *current* would be directly produced through the *pond*.

Exp. 5. After having disturbed the state of the *divergences* at the extremities of the *column*, by placing a finger on one of them, the mode of reproducing speedily the former state is, to lay the fingers on both extremities together, and remove them *at the same instant*: if this last condition be really obtained (which is not easy) the original *divergences* are restored. Exp. 5.

These experiments cannot leave any doubt, that the phenomena of the *column*, as well as the *electric* part of those of the *pile*, are produced by a *fluid* set in *motion*. A fluid set in motion.

Exp. 6. In the same case as that of the above experiments, namely, when the *divergences* are not too great, if one side of the *column* be placed in communication with the ground, the effects of the contact on the other side are so similar to those produced in the same manner on the *leaves* of the *mimosa sensitiva*, that this conformity of effects seems to indicate some analogy between the causes: both contacts make the *leaves* fall; they rise again, but it requires some time. There is an objection against the idea that the phenomenon of the plant is *electric*; because in the *column* the same effect is produced, at one side by *imparting*, and at the other by *taking off*, some *electric fluid*. This objection however is not absolute, for we do not know all the actions of *organic bodies* on the *electric fluid*; but if it is not this, it must be some other *fluid*, which is acted upon by the contact of the plant. In general, we are very little advanced in the knowledge of the subtile *agents* operating in terrestrial phenomena; and as we cannot make any real progress in this knowledge but by endeavouring to increase, by observation and experiments, the number of the phenomena which have analogies with each other in some respect, it might be useful to follow an attentive comparison, at different times and in different arrangements of circumstances, between Exp. 6. Effect like those on the leaves of the sensitive plant.

between the effects of contact on the *gold leaves* of the *column*, and on the *leaves* of the *mimosa sensitiva*, and even contact with different bodies.

Stronger proofs of a moving fluid.

I come to greater symptoms of the *motion* of the *electric fluid* in the *column*, beginning by experiments, which will prove what, in the former *paper*, I have concluded, from my theory, namely, that by the cause assigned to this *motion*, the *negative* effect goes on increasing from the *zinc* to the *copper* extremity of the *column*, at the same time that the *positive* effect increases from the latter to the former; and that the *electric state* of each point of the insulated *column* is the *sum* of the correspondent terms of two inverse series of progress represented by determined, though in some respects variable, numbers, in a *table* given in that *paper*.

Addition to the apparatus.

For these experiments a third *electroscope* is used: in the *figure* it is seen connected with the middle point C of the *column*, where is a thick brass plate with a projecting loop, 4. This immediate connexion of the electroscope with the middle point of the *column* serves for some experiments; but every other point of the *column* may be made to communicate with it, by the interposition of a soft wire held in the middle by an insulating handle. When this is used, the communication of the electroscope with the middle point is taken off; and, by bending the wire, it is easily made to connect, as *conductor*, the necessary parts of the *column* with this *electroscope*.

Exp. 7.

Exp. 7. At a time when there are simple *divergences* in the electroscopes at the extremities of the *column*, if they be equal, *positive* at A, and *negative* at B, there is no *divergence* in the electroscope at C; this is *neutral*; which is the case expressed in *Table I* of the former paper: and if at this time any point of the *column*, at a distance from the point C, on the *negative* or *positive* side, proportional to one of the terms of the *table*, be tried with the insulated *conductor*, the *divergence* which it produces will be found, as exactly as can be expected in such experiments, correspondent to the *number* expressed in the *table*, with its *sign*.

Exp. 8.

Exp. 8. In this situation of the *column*, the states expressed in *Table II* and *Table III* may be also observed by

by placing alternately its extremities in communication with the *ground*; but by a *wire*, because the metallic *chains*, commonly used for this purpose, do not transmit completely such small quantities of *electric fluid*, probably on account of some dust getting between the links. The following are the two cases of this experiment.

A wire conducts better than a chain.

1. When the communication with the ground is made at B, the electroscope at the middle point C diverges *positively*, in the same degree as did before the electroscope at A; and the divergence of the latter is now nearly *double*. I say nearly, because equal increases in the *electric state* produce smaller increases in the *angular* motion of the *gold leaves* in proportion as the *angle* increases. By using then the small insulated *conductor*, it is found, that the whole *column*, (except the very extremity B, which, communicating with the *ground*, is *neutral*) is in a *positive* state, increasing towards A: which is expressed in *Table II.*

Divergence of the electroscope not in the precise ratio of the intensity.

2. When the communication with the *ground* is made at A, the *electric states* of the *column* are all reversed. The electroscope at the middle point C has now a *negative* divergence, equal to that of the electroscope at B in the insulated state of the column; and the divergence at B is nearly double. Then, by observing the state of the other parts of the column with the insulated *conductor*, the *negative* state is found increasing toward B, from A, the only point not *negative*, but *neutral*. This is the case represented in *Table III.*

In the three different cases above described, the indicated *positive* and *negative* states are, in every part of the *column*, common to *zinc* and *copper*. There is no doubt, in every binary association of the *metals*, that *difference* between them which their nature requires; but it is insensible in them individually, as it is when they are single; and their *electric state*, embracing both *metals*, is determined, according to their position in the *column*, by the *motions* of the *electric fluid* resulting from these insensible *elements*; and that they follow the laws determined in my paper from the cause assigned, is verified by these experiments; which demonstrate at the same time, that there are no *positive* or *negative* states belonging to any part of the *column* (nor consequently

Both states of electricity common to both metals.

consequently of the *pile*); since each part may change from *positive* to *negative*, or inversely, according to circumstances, by the different *motions* impressed on the *electric fluid*; which *motions* may be concluded from these phenomena themselves, but will be directly perceived in the following experiment.

Exp. 9. *Exp.* 9. A necessary condition of this class of experiments is, that the state of the *ambient air* be such, that alternately, at each extremity of the *column*, one of the *gold leaves* strikes the side of the electroscope, and at last *sticks* at one of the extremities. The following are three different cases in these phenomena.

1. When the *strikings* are alternate at the extremities, these instantaneous communications of the *column* with the *ground* at each side, by the contact of the *gold leaf* with the *tin foil*, produce in the former a *flux* and *reflux* of *electric fluid*. When the *gold leaf strikes* at the *copper* extremity, some *fluid* ascends from the *ground* into the *column*, and repairs the deficiency on this side; but this additional quantity of *electric fluid* in the *column* occasioning a *striking* at the *zinc* extremity, the new quantity of *fluid* returns that way to the *ground*. These *rises* and *ebbs* of the *electric fluid* in the *column* are observed at the *middle electroscope*, but only when some time elapses between the *strikings*; for, on account of the *slowness* of *motion* of the *fluid*, directly shown above, when the *strikings* rapidly alternate, before one of the effects has extended itself in the *column*, the contrary effect begins; in the same manner as the *rise* and *ebb* of the water are not sensible within the *Mediterranean sea* and the *Baltic*, on account of their narrow entrance; for before the *flux* has extended itself some way up these seas, the *reflux* operates in the contrary direction. But these *motions* of the *electric fluid* are very sensible at the *middle point*, in the following cases.

Cause of the want of tides in the Mediterranean and Baltic.

2. When the *gold leaf* comes to adhere at the *copper* extremity, thus placing it in a continued communication with the *ground*, the *strikings*, which become more frequent at the *zinc* side, produce a *pulselike stream* of the *electric fluid* in the *column*, manifested by the motions of the *gold leaves* in the *middle electroscope*: their divergence is *positive*, the

the whole *column* being now in this state (*Exp.* 8, 1.), but they *fall* in part, when the *gold leaf* strikes at the *zinc* side, and *rise* in the intervals of the strikings; thus pointing out clearly a *current* flowing from B to A, at a *higher* level than the *standard*, which level alternately *rises* and *falls*.

3. When the adhesion of the *gold leaf* takes place at A, the *zinc* side; which circumstance, producing a continued communication of this side with the *ground*, renders the column *negative* in the whole (Exp. 8, 2.); while the *gold leaf* at B, the *copper* side, goes on *striking*; a *current* of *electric fluid* is also produced, but at a *lower* level than the *standard*: the divergence of the *gold leaves* in the *middle electroscope* is consequently *negative*, and, as in the former case, they *fall* in part at every *striking*, and *rise* in the intervals; but while in that case they *fell* by the *lowering* of level of the *current*, and *rose* when it came *higher*; now diverging as *negative*, they *fall* at every *striking*, because some *fluid*, ascending from the *ground*, makes the column less *negative*; and they *rise* again while this fluid flows into the *ground* by the *zinc* side, and thus prepares another *striking*.

Motions of the fluid in the pile more confused and less lasting.

I have made the same experiments on the *motions* of the *electric fluid* within the *pile* itself; they are more confused and less lasting on account of the calcination of the metals: but the *column*, being in fact the *electric machine* of the *pile*, shows clearly and permanently these *motions*, which I shall now follow in the *circuit*, or when the two extremities of the *column* (or the *pile*) are connected together by some intermediate body. In this case the *motion* of the *electric fluid* is manifested by more or less *retardation* of its *current*, according to the degree of *conducting* faculty of the body employed.

Additional apparatus.

For this class of experiments (see the *figure*) brass hooks, 5 and 6, are fixed to the small brass plates, terminating the *column* at each extremity, and against which press the screws: these hooks project a little more than an inch, and serve for different purposes. The following experiments will relate to the *conducting* faculty of that kind of *glass tube* filled with *water*, entered by *wires* on both sides, in which *chemical* effects are produced when it is applied to the

the *pile*; but with the *column*, these effects do not take place.

Exp. 10. *Exp.* 10. The tube of the above kind, which serves in this experiment, is represented in the *figure*, as suspended at the point 7; its wire, 8, having a hook, held up by a silk thread, which, passing over the pulley, 10, descends to a thin brass plate, 11, fixed to the base of the instrument. This brass piece bends forward at the top, and the silk, entering into a notch of this projection, is stopped there by a bead fixed to it. The other wire, 9, of the tube, is hooked on the projecting wire, 6. In this situation of the *tube*, it does not affect the *electroscopes* of the *column*, they continue to diverge as if the *tube* were not connected with one of them; but when, the silk being disengaged, the hook of the wire, 8, comes to rest on the hook, 5, of the *column*, the *circulation* of the *electric fluid* produced through it between the extremities A and B is so rapid, that the *divergence* entirely ceases in the *electroscopes*; and it returns only, when the extremity of the wire, 8, is again lifted up. This shows, that the *glass tubes* of this kind are sensibly as good *conductors* as *metals*.

Permeability of bodies to electricity. The different *conducting* faculties of bodies proceed from different degrees of *adhesion* of the *electric fluid* to them; but beside this difference among bodies, there is another, which relates to *permeability*. All the bodies, which I have tried, are *permeable* to the *whole* of the *electric fluid*, except those that can be *charged*; which are *impermeable* to the *electric matter*, and *permeable* only to the *vector*. This

Charged bodies. operation, called *charge*, as I have explained and proved in my works, consists in accumulating the *electric matter* only on one side of the *laminæ* made of these substances, by occasioning a proportional diminution of its quantity on the opposite surface, which is an operation of the *vector*; and the reason why other bodies cannot be *charged* is, that, the *electric matter* pervading them, though slowly in some of them, no sensible difference in the quantity of *electric matter* can be produced between their opposite surfaces.

Glass impermeable to the fluid, but it moves along its surface. *Glass*, in this respect, is a remarkable substance. It is absolutely *impermeable* to the *electric matter*, and, being a solid body, it is used for insulating pillars in our electric apparatusses;

apparatusses; but the *electric matter* moves easily along its surface, as I have visibly shown by the *Lichtenberg figures* produced on its naked surface, where they dissipate in a little time, while they last many days on *resinous* surfaces. This is the reason of covering the *glass* pillars destined to insulate with some *resinous* varnish; but all these varnishes are not equally fit for the purpose, and this is one of the objects of the following process, as well as the trial of the different *conducting* faculties of other bodies.

Varnishes differ in properties.

For these experiments, the bodies to be tried must be reduced into slips or rods, which are to be laid on the hooks, 5 and 6, of the *column*, in order to observe the effect produced on its *electroscopes*; but there are necessary precautions to be used in laying them on the hooks. For instance, in respect to the bodies with which I shall begin, those which have different *conducting* faculties, belonging mostly to the vegetable and animal kinds, when they are placed on the hooks with the *fingers*, as it is almost impossible to lay them on both hooks at the same instant, the end which touches first disturbs the *equilibrium* of the *electric fluid* in the *column*; and I have shown above, that it is but slowly restored. In order to obviate this defect, and for another purpose that will follow, two brass wire brackets, 12, 12, are fixed in the front of the base of the instrument, on which the slips are first laid, and there taken up by two glass hooks covered with sealing wax, with which they are placed on the hooks of the *column*. I shall give a general idea of these trials under the following head.

Trials of the conducting powers of bodies.

Exp. 11. The substances of this class having more or less *conducting* faculty, they lessen in different degrees the *divergence* in the electroscopes, by transmitting more or less *electric fluid* from A to B. This is a curious kind of experiments, but as the particulars are not here my object, I shall relate only one, concerning the physiology of vegetables, which may lead to others of the same kind. Having repeated in presence of Dr. Lind these experiments on the different *conducting* faculties of various bodies, I showed him a phenomenon, which had surprised me. A thin slip of *deal*, cut *along* the *fibres*, being applied to the *column*, there remained but little *divergence* in the electroscopes; while a slip of the same wood, of the same thickness and breadth,

Exp. 11.

Difference in deal cut lengthwise & across.

breadth, but cut *across* the *fibres*, produced much less diminution in the *divergence*. Dr. Lind found probably the cause of this difference, assigning it to the situation of the *resinous* substance within that wood: it does not belong to the *fibres* themselves, since they transmit easily the *electric fluid*; it is lodged between the *fibres*, and thus forms an impediment to the passage of the *fluid* from *fibre* to *fibre* in the slips cut *across* them.

Trials on the insulating powers of bodies.

When these experiments are made with the view of trying the *insulating* property of bodies, still more precaution is required in placing them on the hooks of the *column*: for the bodies fit for this use being fundamentally *impermeable* to the *electric matter*, their *electric state* is changed, more or less permanently, by *friction*; and this in the manner which I intend to explain in a future paper. As, however, they can hardly be handled without some *friction*, they act upon the column by their *influence* (an effect that I shall show directly hereafter), and their *insulating* property cannot be observed, on account of the disturbance which they produce in the state of the *column* itself. These bodies therefore must remain a little time *untouched* on the brackets, and be there breathed upon, in order that the *moisture* of the breath may dissipate their *electrization*; serving as a *conductor* for their whole surface to the ground, through the brackets; and when the *moisture* is evaporated, the rods are taken up there with the insulating hooks, and thus applied to the *column*.

These experiments are particularly useful for a better knowledge of *insulation*, a point very important in the construction of electric apparatusses; for many experiments fail for want of a complete *insulation*; and I do not know of any shorter and surer method of trying the *insulating* faculty of *varnishes* laid on glass for this purpose, than that of applying to the *column* rods of *glass* covered with them. I shall also give only a general idea of this class of trials under the following head.

Exp. 12.

Exp. 12. It is very seldom, that a naked *glass* rod, being placed on the hooks of the *column*, does not sensibly diminish, in a little time, the *divergence* in the electroscopes, by transmitting slowly some *electric fluid* from A to B: but this is more or less, according to the nature of the

the glass; therefore these differences may become a particular object of experiments, concerning the property of different *glasses* for electric purposes, by comparing the effects of different rods on the electroscopes of the *column*; much care being taken, that all effect of *friction* be dissipated. With respect to *varnishes*, a proper *sealing wax* laid over the *glass* when *hot* is the best coating which I have found; a rod of this kind produces no change in the electroscopes. But all *sealing waxes* have not the same property, and before I had devised this mode of trial, I was sometimes disappointed in the construction of the small set of electric apparatusses, which I have mentioned in the former paper, even in that of the *electroscopes* of the *column*, as the top of their *glass* bottle must be *insulating*. The *sealing wax* reckoned the *finest*, because it melts more easily and spreads more smoothly, is not fit for this purpose, its softness being produced by spirit of wine. In general, for this essential choice of an *insulating* coating on *glass*, the column is very useful; for by laying the different *coatings* on *glass* rods, and placing these on the hooks of the *column*, those which will be found to diminish the *divergence* in the electroscopes are not completely *insulating*, and that *coating* must be used, by which the *divergences* are not affected.

Sealing wax the best varnish,

but not the finest.

I come to the impression produced on the *column* itself, when there remains some effect of *friction* on the *insulating* rods applied to it. The experiments on this object will, at the same time, afford a new verification of the cause which I have assigned to the *motion* of the *electric fluid* in the *column* (or the *pile*), by what is called the *electric influence*; the *laws* of which, first really determined by Sig. Volta, I have explained by some modifications of the *vector*, which will be perceived in the following experiments; showing at the same time the effect of the *ambient air*, such as I have determined it. The proper time for these experiments is also when, from the *electric state* of the *ambient air*, there is not much *divergence* in the electroscopes of the *column*. They may be made with naked *glass*, which *friction* renders *positive*; and with a *glass* rod covered with *sealing wax*, which thus becomes *negative*. I shall explain the phenomena

Effects of friction on the insulating rods.

mena produced by the latter, because it retains longer the effect of *friction*; those produced by naked *glass* are of the same nature, only reversed.

Exp. 13. *Exp.* 13. A glass rod covered with *sealing wax* must be rubbed so gently, that, when applied to the *column*, it only increases the *divergence* of the gold leaves at the *negative* side, without any *striking*, else the effects would be confused: when it produces the proper effect, the phenomena are the following.

1. At the moment when the *rod* is placed, the *divergence* increases at the *negative* side, and diminishes, or even ceases at the *positive* side. The cause of these effects is, that the *expansive power* of the *electric fluid* is lessened in the whole *column*, by part of its *vector* passing to the *negative* rod, where its quantity is *less:* and as however the *equilibrium* of the *electric fluid* in the *column* requires more *electric matter* on *zinc* than on *copper*, the latter, at the first moment, loses more of it, but not sufficiently to compensate the loss of *expansive power* at the *zinc* side: therefore less *electric fluid* passes into the *gold leaves* of the latter, which fall in consequence of this diminution.

2. Within a little time, the *positive* divergence is renewed at the *zinc* side, and the *negative* lessened at the *copper* side. This effect is produced by the *ambient air*, which, during the diminution of *expansive power* in the *column*, yields to it some *electric fluid*, especially to the *copper* side which was become strongly *negative;* and thus the former *equilibrium* is restored in the *column*.

3. The proof of the above explanations of the phenomena observed, which embrace the whole system, both of the *motion* of the *electric fluid* in the *column*, and of the influence of the *ambient air* on this instrument, is obtained by suddenly removing the *rod*: for the new quantity of *electric fluid* communicated to the *column* by the *air* during the *influence* of the *negative* body makes both *gold leaves* strike at once on the *zinc* side by a very great *divergence*.

Effects of differences in the size & number of plates. The last object concerning the theory of the *electric effects* in both the *column* and the *pile*, which remains to be considered, relates to the difference between the effects of the *number* of *groups* and the *size* of the *plates*, considered under

under two distinct points of view, on one of which I have already given an experiment in my former paper; but I have repeated it in a different manner, which will confirm my system on these phenomena.

Exp. 14. The *column* of 600 *groups*, represented in the *figure*, has been composed of three *columns* of 200 *groups* each, which I had used separately from the beginning of these experiments; but before they were united together for the purpose of the *aerial electroscope*, I tried their effects in the three following combinations. Exp. 14.

1. I applied successively to the same *electroscope* the same extremity (either *positive* or *negative*,) of each *column*, the opposite extremity being placed in communication with the ground; and I observed the quantity of *divergence* produced by each *column*, which was nearly the same.

2. I applied the three *columns* at the same time to the *electroscope*, each of them remaining in communication with the ground; and the *divergence* was not greater than it had been with the most active of the single *columns*.

3. But having connected the three *columns* as one, by placing conductors between their opposite extremities, and connecting one extremity of the whole with the electroscope, the other being in communication with the ground; the effect was so much increased, that the *gold leaves* struck the sides of the electroscope.

Number of plates analogous to the length of a pump for raising water.

This proves, under a different form, the same proposition which I had stated in my former paper, namely, that the simple *divergence* in electroscopes depends only on the *density* of the *electric fluid*, and the *density* on the *number* of *groups*; at the same time that it confirms the cause which I had assigned to these effects; and as they are analogous to many kinds of phenomena, I shall use another example to explain it, that of *pumps*. As the *height* to which water can be raised by *pumps* does not depend on either their *number* or *size*, but on their *length*; so in the above experiment, with three concurrent *columns* of 200 groups, the *density* of the *electric fluid* was not increased on one extremity, nor consequently the *divergence* at either extremity, more than with one *column;* nor could more have been done with one column of the same *number* of *groups*

of whatever *size*. But as, in taking the water at the same level, a *pump* of 30 feet will bring it three times as *high* as three *pumps* of 10 feet; so in the above experiment, the *column* of 600 *groups* produced probably three times as much change of *density* in the *electric fluid*, with a proportional *divergence*, as did the three *columns* of 200 groups, individually acting on the same low *level*, or degree of *density*, that of the *standard* of *plus* or *minus*.

and their size to that of the bore or number of pumps.

But if in this case the *size* of the *plates*, or the multiplication of their *number* at the same *numerical* distances from the extremities, be indifferent, it is not the same in some other cases, as I shall illustrate by the same analogy. In the above case, the *height* to which the *water* was to be raised being the only object, the number or size of the *pumps* was indifferent; but if a *current* is to be produced at that *level*, either with a certain degree of rapidity, or of a certain volume, then the *diameter* of the *pumps* comes in as a condition. The following experiment will show the analogy of this case, with the effects of the different *sizes* of *plates* in the *column*.

Exp. 15.

Exp. 15. I made two other *columns* of 200 groups each; but these I only cut square, for one of $\frac{1}{4}$, and for the other of $\frac{1}{8}$ of an inch, still *zinc* and *Dutch gilt paper*. These two *columns* produced sensibly the same *divergence* as the former, in the same electroscope; but in this was already shown the difference in other respects; the *time* for producing this *divergence* was in the inverse ratio of the *size* of the *plates*.

This experiment gives a clear idea of the effect produced by a greater *size* of the *plates*, both in the *pile* and in the *column*. In the *circuit* of the former, with the same *number* of *groups*, the effects are proportional to the *size* of the *plates*, because the *current* of the *electric fluid* becoming denser and more rapid in passing through the *wires* used in these operations, the effects are greater, in proportion to the number of *equal parts* of surface, either in a few or many *plates*, concurring to produce the *motion* of the *electric fluid* which arrives at the entrance of this narrow channel. That difference in the rapidity and density of the *current* cannot be discovered in the *circuit* of the *column*, because

: the condition to which *chemical effects* are owing is
; in it, as I have explained in my former paper; but
of the *plates* influences the *frequency* of the *strikings*
ittle electroscopic *pendula*, when their simple *diver-*
s exceeded; because each time that one of them
either at the *negative* or the *positive* side, that in-
ous communication of the *column* with the ground
in some degree its *electric* state; and the same state
r restored, to produce another *striking*, in propor-
the *size* of the *plates*, with the same *number* of *groups*.
fect will enter as an essential circumstance into the
t of this paper, concerning the *aerial electroscope*.

II.

kerian Lecture for 1809. *On some new Electro-*
cal Researches, on various Objects, particularly the
lic Bodies from the Alkalis, and Earths, and on
Combinations of Hidrogen, By HUMPHRY DAVY,
Sec. R. S. F. R. S. E. M. R. I. A.

(Concluded from p. 55.)

IV. *On the Metals of Earths.*

/E tried a number of experiments, with the hopes ng the same distinct evidences of the decomposition common earths, as those afforded by the electro- l processes on the alkalis, and the alkaline earths. l, that, when iron wire ignited to whiteness, by the of 1000 double plates, is negatively electrified and n contact with either silex, alumine, or glucine, moistened and placed in hidrogen gas; the iron be- rittle and whiter, and affords by solution in acids an the same kind, as that which has been employed in eriment.

Metals of the common earths

apparently combined with iron.

e passed potassium in vapour through each of these heated to whiteness in a platina tube: the results markable, and perhaps not unworthy of being fully l.

Potassium in vapour

passed through silex,

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali formed upon the potassium. The potassium* was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence; but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an hour; so that there is great reason to believe, that the silex had been either entirely or partially deoxigenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left, as the result of its combustion, alkali and silex. The part which did not inflame was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

alumine, and glucine.

Potassium, in acting upon alumine and glucine, produced more hidrogen than could be ascribed to the moisture present in the crust of potash; from which it seems probable, that, even after ignition, water adheres to these earths.

The results of the action of the potassium were pyrophoric substances of a dark gray colour, which burnt, throwing off

* The results of this experiment are opposed to the idea, that potassium is a compound of hidrogen and potash, or its basis; for, if so, it might be expected, that the hidrogen would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination, I operated in an apparatus connected with water, and I found, that the potassium produced as much hidrogen, as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapour of the water, which must have been constantly supplied.

brilliant

brilliant sparks*, and leaving behind alkali and earth; and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small, that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them, which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

Attempt to form alloys with them.

Supposing the metals of the earths to be produced in experiments of this kind, there was great reason to expect, that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance, which it was safe to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury while the tube was hot; but the alkaline metal gave the characters to the amalgam: and though, in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied, that there was any of the metals of these earths in triple combination.

Mixtures of the earths, potassium, and iron filings, formed alloys.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded by solution in

* The pyrophorus from alum, which I have supposed in the last Bakerian lecture to be a compound of potassium, sulphur, and charcoal, probably contains this substance likewise. Homberg's pyrophorus.

acids,

acids, evaporation, and treatment with reagents, oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Amalgams formed with the alkaline earths.

Though I could not procure decided evidences of the production of an amalgam from the metals of the common earths, yet I succeeded perfectly by the same method of operating in making amalgams of the alkaline earths.

Lime, and magnesia.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

Amalgam of magnesia.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder; and which, when acted upon by weak muriatic acid, gave off hidrogen gas in considerable quantities, and produced a solution of magnesia.

Proportions of metal in the earths may be obtained.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxigen which they absorb; and by the solution of the alloys containing the metals of the common earths, it seems probable, that the proportions of metallic matter in these bodies may likewise be ascertained.

Hypothetical calculation.

On an hypothesis which I have before brought before the Society, namely, that the power of chemical attraction and electrical action may be different exhibitions of the same property of matter; and that oxigen and inflammable bodies are in relations of attraction, which correspond to the functions of being negative and positive respectively; it would follow, that the attractions of acids for salifiable bases would be inversely as the quantity of oxigen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxigen and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808 concluded, that barytes must contain the least oxigen of all the earths; and that the order, as to

the

the quantity of inflammable matter, must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxigen of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90·5* of metal per cent, strontites 86†, lime 73·5*, magnesia, 66‡. Inferences.

The same proportions would follow from an application of Mr. DALTON's ingenious supposition§, that the proportion of oxigen is the same in all protoxides; and that the quantity of acid is the same in all neutral salts, i. e. that every neutral salt is composed of one particle of metal, one of oxigen, and one of acid. Composition of protoxides and neutral salts.

* Mr. James Thompson, Nicholson's Journal, vol. xxiii, p. 175, and Berthier.

† Mr. Clayfield. Thompson's Chemistry, vol. ii. p. 626, 629.

‡ Murray's Chemistry, vol iii, p. 616.

§ The principle, that I have stated, of the affinity of an acid for a salifiable basis being inversely as the quantity of oxigen contained by the basis, though gained from the comparison of the electrical relations of the earths with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. Dalton's law of general proportions. Mr. Dalton had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalis and alkaline earths; which, in the case of the alkalis, were not very remote from what I had ascertained by direct experiments. Mr. Gay-Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxigen, might (as far as it is correct) be inferred from Mr. Dalton's law; though this ingenious chemist states, that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxigen, the same in all protoxides; and there is a proportion of acid, the same in all neutral salts; and new proportions of oxigen and of acid are always multiples of these proportions. So that, if a protoxide, in becoming a deutoxide, takes up more acid, it will be at least double the quantity; and in these cases the oxigen will be strictly as the acid. Mr. Dalton's law even provides for cases, to which Mr. Gay-Lussac's will not apply; a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxisulphat of iron perfectly formed, (as some experiments, which I have lately made, seem to show,) there is probably only a single proportion of acid; and in the supertartrite of potash there is only a single quantity of oxigen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

We

Proportion of metal in alumine,

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex; but according to Richter's estimation of the composition* of phosphate of alumine, alumine would appear to contain about 56 per cent of metallic matter.

and silex.

Mr. Berzelius, in a letter which I received from him a few months ago†, states, that, in making an analysis of cast iron, he found, that it contained the metal of silex; and that this metal, in being oxidated, took up nearly half its weight of oxigen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47‡ of oxigen, which agrees very nearly with the quantity of hidrogen and ammonia produced from the amalgam.

Earths formerly considered of the same class with oxides: afterward alkalis, earths, and oxides, deemed separate orders,

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of combining with phlogiston; and though Lavoisier insisted upon this analogy with his usual sagacity; yet still the alkalis, earths, and oxides, have been generally considered as separate natural orders. The earths, it has been said, are not precipitated by the triple prussiates, or by the solutions of galls§; and the alkalis and alkaline earths are

but apparently without reason.

both distinguished by their solubility in water: but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalis and alkaline earths combine

* Thomson's Chemistry, vol. ii, p. 581.

† In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

‡ I take the proportions of the volumes from the very curious paper of Mr. Gay-Lussac, on the combinations of gaseous bodies, Mem. d'Arcueil, tom. ii, page 213; and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by Messrs. Gay-Lussac, and Thenard.

§ Klaproth. Annales de Chimie, tom. x, p. 277.

with

with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shown by Dr. Henry and Mr. Guyton,) precipitate the triple prussiate of potash: the power of combination is general, but the compounds formed are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutrosaline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalis and earths with oils, to form soaps; and of the earthy soaps some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxigen, approach very near in their general characters to zircon, silex, and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalis approach to the lightest class of oxidable metals?

It will be unnecessary, I trust, to pursue these analogies any farther; and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

Alloys formed in metallurgical operations, which affect the qualities of the metals: as iron,

It is probable, that these alloys may be formed in many metallurgical operations; and that small quantities of them may influence materially the properties of the compound, in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold short iron; while, from another spot, it will make hot short: but by a combination of the two in due proportions, good iron is produced. May not this be owing to the circumstance of their containing different metals of the earths, which in compound alloys may be more oxidable than in simple alloys, and may be more easily separated by combustion?

and copper.

Copper, Mr. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of

of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

Speculations on the nature of hidrogen,

Hidrogen is the body which combines with the largest proportion of oxigen, and yet it forms with it a neutral compound. This on the hypothesis of electrical energy would show, that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxigen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phenomena presented by sulphuretted hidrogen are of the same kind, and lead to similar conclusions.

muriatic acid gas,

Muriatic acid gas, as I have shown, and as is farther proved by the researches of Messrs. Gay-Lussac and Thenard, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decomposed, unless a new combination is formed: thus it is not changed by charcoal ignited in the gas by Voltaic electricity; but it is decomposed by all the metals; and in these cases hidrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxigen being found in the new compound. This at first view might be supposed in favour of the idea, that hidrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitromuriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxigenation.

and nitrogen.

That nitrogen is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia;

ammonia; and (even supposing no reference to be made to the experiments detailed in this paper,) the general analogy of chemistry would lead to the notion of its being compounded.

Are hidrogen, ammonia, and nitrogen, oxides of the same metal?

Should it be established by future researches, that hidrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal; the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus*, and of the boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to the suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter to acid matter might be supposed capable of being ascertained by their relations to oxigen, or by the peculiar state of their electrical energy.

Sulphur and phosphorus appear to contain hidrogen.

* The electrization of sulphur and phosphorus goes far to prove, that they contain combined hidrogen. From the phenomena of the action of potassium upon them in my first experiments I conceived, that they contained oxigen; though, as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process appeared an evidence in favour of their containing oxigen, till I discovered, that similar phenomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hidrogen, and on phosphuretted hidrogen, I find that the phenomena differ very much according to the circumstances of the experiment; and in some instances I have obtained a larger volume of gas from potassium, after it had been exposed to the action of certain of these bodies, than it would have given alone. These experiments are still in progress, and I shall soon lay an account of them before the Society. The idea of the existence of oxigen in sulphur and phosphorus is however still supported by various analogies. Their being nonconducters of electricity is one argument in favour of this. Potassium and sodium, I find, when heated in hidrogen, mixed with a small quantity of atmospheric air, absorb both oxigen and hidrogen, and become nonconducting inflammable bodies analogous to resinous and oily substances.

The

The whole tenour of the antiphlogistic doctrines necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which, if not so simple, account for the phenomena with at least equal facility.

Phlogistic hypothesis. If hidrogen, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxigen, hidrogen, and unknown bases only; metals and inflammable solids will be compounds of these bases with hidrogen; the earths, the fixed alkalis, metallic oxides, and the common acids, will be compounds of the same bases, with water.

Arguments in favour of this hypothesis. The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are, First, The properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hidrogen or oxigen, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hidrogen, whether in its simple form, or in combination with one proportion of oxigen, or a double proportion, from my experiments, seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hidruret, seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

d. Second. The facility with which metallic substances are revived, in cases in which hidrogen is present. I placed two platina wires, positively and negatively electrified from 500 double plates of 6 inches, in fused litharge; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed. The difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable

to

to the idea, that the presence of hidrogen is essential to the production of the metal.

Third. Oxigen and hidrogen are bodies, that in all 3d.
cases seem to neutralize each other; and therefore, in the products of combustion, it might be expected, that the natural energies of the bases would be most distinctly displayed, which is the case; and in oximuriatic acid, the acid energy seems to be blunted by oxigen, and is restored by the addition of hidrogen.

In the action of potassium and sodium upon ammonia, Arguments against it.
though the quantity of hidrogen evolved in my experiments is not exactly the same, as that produced by their action upon water; yet it is probable, that this is caused by the imperfection of the process*: and supposing potassium and sodium to produce the same quantity of hidrogen from ammonia and water, the circumstance at first view may be conceived favourable to the notion, that they contain hidrogen, which, under common circumstances of combination, will be repellent to matter of the same kind; but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for, on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then, whenever bodies known to contain hidrogen are decomposed by a metal, the quantities of hidrogen ought to be the same, or multiples of each other. Thus in the decomposition of ammonia by potassium and sodium, two of hidrogen and one of nitrogen remain in combination, and one of hidrogen is given off; and in the action of water on potassium to form potash, the same quantity of hidrogen ought

* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hidrogen evolved; i. e. whenever the metals of the alkalis act upon ammonia, (supposing this body to be composed of 3 hidrogen, and 1 of nitrogen, in volume, 2 of hidrogen and 1 of nitrogen remain in combination, and 1 of hidrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalis and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

to

to be expelled. From my analysis* of sulphuretted hydrogen it would appear, that, if potassium in forming a combination with this substance sets free hidrogen, it will be nearly the same quantity, as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett of the sulphuret of iron be made a basis of calculation, iron, in attracting sulphur from sulphuretted hidrogen, will liberate the same proportion of hidrogen as during its solution in diluted sulphuric acid; and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hidrogen, because in acting upon different combinations containing hidrogen they produce the evolution of equal proportions of this gas, then it might be proved, that almost any kind of matter is contained in any other. The same

Composition of sulphuretted hidrogen.

* The composition may be deduced from the experiments in the last Bakerian lecture, which show, that it contains a volume of hidrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2·27 of hidrogen, and 32·73 of sulphur. When sulphuretted hidrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hidrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hidrogen, it was found, that, when these gasses were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common states; the phosphorus was dark coloured, and the arsenic appeared as a brown powder: both were probably hydrurets. This is confirmed likewise by the action of potassium upon arseniuretted and phosphuretted hidrogen: when the metal is in smaller quantity than is sufficient to decompose the whole of the gasses, there is always an expansion of volume; so that arseniuretted and phosphuretted hidrogen contain in equal volumes more hidrogen than sulphuretted hidrogen, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hidrogen, it would appear, that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

quantity

quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

Potassium, sodium, iron, mercury, and all metals, that I have experimented upon, in acting upon muriatic acid gas evolve the same quantity of hidrogen, and all form dry muriates; so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The nature of the amalgam of ammonia of importance in deciding the question.

The solution of the general question concerning the presence of hidrogen in all inflammable bodies will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxigen, which may be supposed to exist in hidrogen, that might be applied to explain the composition of nitrogen from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hidrogen and nitrogen; or that a substance which is metallic can be composed from substances not in their own nature metallic, is equally opposed to the general tenour of our chemical reasonings.

I shall not at present occupy the time of the Society, by entering any farther into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research have not been sufficiently examined, to enable us to decide upon their nature, and their relations, or to form any general theory concerning them, which is likely to be permanent.

III.

The Croonian Lecture. On the Functions of the Heart and Arteries. By THOMAS YOUNG, *M. D. For. Sec. R. S.*

(Concluded from p. 68.)

Functions of the muscular fibres of the coats of the arteries.

They have less effect on the motion of the blood, than is generally supposed.

Arguments to prove this.

I Shall proceed to inquire, in the third place, into the nature and extent of the functions, which are to be attributed to the muscular fibres of the coats of the arteries; and I apprehend, that it will appear to be demonstrable, that they are much less concerned in the progressive motion of the blood, than is almost universally believed. The arguments, which may be employed to prove this, are nearly the same that I have already stated, in examining the motion of a fluid, carried along before a moving body in an open canal; but in the case of an elastic tube, the velocity of the transmission of an impulse being rather diminished than increased by an increase of tension, the reasoning is still stronger and simpler; for it may here be safely asserted, that the anterior parts of the dilatation, which must be forced along by any progressive contraction of the tube, can only advance with the velocity appropriate to the tube, and that its capacity must be proportionate to its length and to the area of its section: now the magnitude of its section must be limited by that degree of tension, which is sufficient to force back through the contraction what remains of the displaced fluid; and the length, by the difference of the velocity appropriate to the tube, and that with which the contraction advances: consequently, if the contraction advance with the velocity of a pulsation, as any contractile action of the arteries must be supposed to do, this length necessarily vanishes, and with it the quantity of the fluid protruded; the whole being forced backwards, by the distending force which is exerted by a very small dilated portion immediately preceding the contraction. It might indeed be imagined, that the contraction follows the pulsation with a velocity somewhat smaller than its own; but this opinion would stand on no other foundation than

Supposition, that the contraction follows the pulsation with less velocity;

mere

mere conjecture, and it would follow, that the pulse would always become more and more full, as it became more distant from the heart; of which we have nothing like evidence: nor would a moderate contraction, even if this supposition were granted, produce any material effect. For example, if the velocity of the contraction were only half as great as that of the pulsation, which is the most favourable proportion, it would be necessary, taking sixteen feet in a second for the velocity of the pulsation, that the section of the arteries should be contracted to about one half, in order to produce, by their progressive contraction only, the actual velocity of the blood in the aorta; one sixteenth of the blood being carried, in this case, before the contraction: but if the contraction were only such, as to reduce the section of the artery to $\frac{9}{10}$, which is probably more than ever actually happens, the velocity produced would be only about $\frac{1}{8}$ as much; and if the contraction were only to $\frac{99}{100}$, which is a sufficient allowance for the smaller arteries, about $\frac{1}{100}$ only of the actual velocity in the aorta could be produced in this manner, even upon a supposition much more favourable to the muscular action of the arteries than the actual circumstances. A small addition must be made to the force required for producing the retrograde motion, on account of the friction to be overcome, but the general reasoning is not affected by this correction.

The contraction of the artery might also be supposed to remain after each pulsation, so that the vessel should not be again dilated until the next pulsation, or, in other words, a spontaneous dilatation might be supposed to accompany the pulsation, instead of a contraction: but such a dilatation would be useless in promoting the progressive motion of the blood, since a larger quantity of blood, conveyed to the smaller vessels, without an increased tension, would be ineffectual with respect to the resistances which are to be overcome. It is possible indeed, that the muscular fibres of those arteries in which the magnitude of the pulse is sensible, like the fibres of the heart, may be inactive, or nearly so, during their dilatation; and that they may contract, after they have been once distended, with a force which is in a certain degree permanent; the greater mo-

or that the contraction remains after each pulsation.

mentum of the blood, which accompanies the dilatation, enabling it to enter the minute arteries with equal ease, although assisted by a tension somewhat smaller: so that the same mean velocity may be sustained, as if the arteries were simply elastic, and a little smaller in diameter, with a very little less exertion of the heart. But the distribution of the blood could never be materially diversified by any operation of this kind: for if any artery were for a moment distended by such a variation, so as to exceed its natural diameter by one hundredth part only, a pressure would thence arise equivalent to that of a column about two inches high, which would, in spite of all resistances, immediately dissipate the blood with a considerable velocity, and completely prevent any local accumulation, unless the elastic powers of the vessel itself were diminished; and this is, perhaps, the most important, as well as the best established inference from the doctrine that I have advanced.

But the distribution of the blood not materially affected in this way.

So that there can be no local accumulation, if the elasticity of the vessels be not diminished.

Circulation in a mola without a heart;

It appears, that a mola has sometimes been found in the uterus, totally destitute of a heart, in which the blood must have circulated in its usual course through the veins and arteries: in this case it cannot be ascertained, whether there was any alternate pulsation, or whether the blood was carried on in a uniform current, in the same manner as the sap of a vegetable probably circulates. If there was a pulsation, it may have been maintained by a contraction of the artery, much more considerable, and slower in its progress than usual; and with the assistance of a spontaneous dilatation; the resistance in the extreme vessels being also probably much smaller than usual: if the motion was continued, it would lead us to imagine, that there may be some structure in the placenta capable of assisting in the propulsion of the blood, as there may possibly be some arrangement in the roots of plants, by which they are calculated to promote the ascent of the sap. The circulation in the vessels of the more imperfect animals, in which a great artery supplies the place of a heart, is of a very different nature from that of the more perfect animals: the great artery, which performs the office of the heart, is here possessed of a muscular power commensurate to its functions, and seems to propel the blood, though much more slowly than in other cases, by

and in animals without a heart,

means

means of a true peristaltic motion. It appears also from the observations of Spallanzani, that in many animals a portion of the aorta, next the heart, is capable of exhibiting a continued pulsation, even when perfectly empty and separated from the heart; but this property is limited to a small part of the artery only, which is obviously capable of being essentially useful in propelling the blood, when the valves of the aorta are closed. The muscular power of the termination of the vena cava is also capable of assisting the passage of the blood into the auricle. It is not at all improbable, that a muscle of involuntary motion, which had been affected throughout the whole period of life by alternate contractions and relaxations, might retain from habit the tendency to such contractions, even without the necessity of supposing, that the habit was originally formed for a purpose to be obtained by the immediate exertion of the muscular power: but in fact the partial pulsation of the vena cava is perfectly well calculated to promote the temporary repletion of the auricle, while it must retard, for a moment, the column which is approaching, at a time that it could not be received.

apparently by a peristaltic motion.

In many animals part of the aorta capable of pulsation.

Muscular power of the end of the vena cava.

Use of its partial pulsation.

There is no difficulty in imagining what services the muscular coats of the arteries may be capable of performing, without attributing to them any immediate concern in supporting the circulation. For since the quantity of blood in the system is on many accounts perpetually varying, there must be some means of accommodating the blood vessels to their contents. This circumstance was very evident in some of Hales's experiments, when, after a certain quantity of blood had been taken away, the height of the column, which measured the tension of the vessels, frequently varied in an irregular manner, before it became stationary at a height proportional to the remaining permanent tension. Haller also relates, that he has frequently seen the arteries completely empty, although in some of his observations there was probably only a want of red globules in the blood which was flowing through them. Such alterations in the capacity of the different parts of the body are almost always to be attributed to the exertion of a muscular power. A partial contraction of the coats of the smaller

Services to which the musular coats of the arteries are adapted.

arteries may also have an immediate effect on the quantity of blood contained in any part, although very little variation could be produced in this manner by a change of the capacity of the larger vessels.

The state of the pulse depends almost wholly on the action of the heart.

According to this statement of the powers which are concerned in the circulation, it must be obvious, that the nature of the pulse, as perceptible to the touch, must depend almost entirely on the action of the heart, since the state of the arteries can produce very little alteration in its qualities.

Modifications of it by the state of the artery.

The greater or less tension of the arterial system may indeed render the artery itself, when at rest, somewhat harder or softer; and, if the longitudinal fibres give way to the distending force, it may become also tortuous: possibly too a very delicate touch may in some cases perceive a difference in the degree of dilatation, although it is seldom practicable to distinguish the artery, in its quiescent state, from the surrounding parts. But the sensation, which is perceived when the artery is compressed, as usual, by the finger, is by no means to be confounded with the dilatation of the artery; for in this case an obstacle is opposed to the motion of the blood, against which it strikes, with the momentum of a considerable column, almost in the same manner as a stream of water strikes on the valve of the hydraulic ram; and in this manner, neglecting the difference of force arising from the different magnitudes of the sections, the pressure felt by the finger becomes nearly equal and similar to that which is originally exerted by the heart: each pulsation passing under the finger, in the same time, as is required for the contraction of the heart, although a very little later; and more or less so, in proportion as the artery is more or less distant; the artery remaining then at rest for a time equal to that in which the heart is at rest. When therefore an artery appears to throb, or to beat more strongly than usual, the circumstance is only to be explained from its greater dilatation, which allows it to receive a greater portion of the action of the heart, in the same manner as an aneurism exhibits a very strong pulsation, without any increase of energy, either in itself, or in the neighbouring vessels; and on the other hand, when the pulsations of the artery of a paralytic arm become feeble,

we

we cannot hesitate to attribute the change to its permanent contraction, since the enlargement and contraction of the blood vessels of a limb are well known to attend the increase or diminution of its muscular exertions. There is also another way, in which the diminution of the strength of an artery may increase the apparent magnitude of the pulse, that is, by diminishing the velocity with which the pulsation is transmitted: for we have seen, that the magnitude of the pulse is in the inverse ratio of the length of the artery distended at once; and this length is proportional to the velocity of the transmission: but it must be observed, that the force of the pulse striking the finger would not be affected by such a change, except that it might be rendered somewhat fuller and softer, although a considerable throbbing might be felt in the part, from the increased distension of the temporary diameter of the artery. How little a muscular force is necessary for the simple transmission of a pulsation may easily be shown, by placing a finger on the vena saphena, and striking it with the other hand at a distant part; a sensation will then be felt precisely like that of a weak arterial pulsation.

Muscular force not required to transmit a pulsation.

The deviations from the natural state of the circulation, which are now to be cursorily investigated, may be either general or partial; and the general deviations may consist either in a change of the motion of the heart, or of the capacity of the capillary arteries. When the motion of the heart is affected, the quantity of blood transmitted by it may either remain the same as in perfect health, or be diminished, or increased. Supposing it to remain the same, the pulse, if more frequent, must be weaker, and if slower, it must be stronger; but this latter combination is scarcely ever observable; and in the former case, the heart must either never be filled, perhaps on account of too great irritability, or never be emptied, from the weakness of its muscular powers. But the immediate effect of such a change as this, in the functions depending on the circulation, cannot be very material, and it can only be considered as an indication of a derangement in the nervous and muscular system, which is not likely to lead to any disease of the vital functions. When the quantity of blood transmitted by the heart

Deviations from the natural state of the circulation,

when the motion of the heart is affected.

heart is smaller than in health, the arteries must be contracted, until their tension becomes only adequate to propel the blood, through the capillary vessels, with a proportionally smaller velocity, and the veins must of course become distended, unless the muscular coats of the arteries can be sufficiently relaxed to afford a diminished tension, which is probably possible in a very limited degree only. In this state the pulse must be small and weak, and the arteries being partly exhausted, there will probably be a paleness and chilliness of the extremities: until the blood, which is accumulated in the veins, has sufficient power to urge the heart to a greater action, and perhaps, from the vigour which it may have acquired during the remission of its exertions, even to a morbid excess of activity. Hence a contrary state may arise, in which the quantity of blood transmitted by the heart is greater than in perfect health: the pulse will then be full and strong, the arteries being distended, so as to be capable of exerting a pressure sufficient to maintain an increased velocity, and to overcome the consequent increase of resistance; a state which perhaps constitutes the hot fit of fever; and which is probably sometimes removed in consequence of a relaxation of the extreme arteries, which suffer the superfluous blood to pass more easily into the veins. Such a relaxation, when carried to a morbid extent, may also be a principal cause of another general derangement of the circulation, the motion of the blood being accelerated, and the arteries emptied, so that the pulse may be small and weak, while the veins are overcharged, and the heart exhausted by violent and fruitless efforts to restore the equilibrium; and this state appears to resemble, in many respects, the affections observed in typhus. When, on the contrary, the capillary vessels are contracted, the arteries are again distended, although without the excess of heat which must attend their distension from an increased action of the heart, and possibly without fever: an instance of this appears to be exhibited in the shrinking of the skin, which is frequently observable from the effect of cold, and in the first impression produced by a cold bath: nor is it impossible, that such a contraction may exist in the cold fit of an intermittent, although it seems more

Hot fit of fever.

Typhus.

Effect of cold.

Cold fit of fever.

more probable, that a debility of the heart is the primary cause of this affection.

Local inflammations.

Beside these general causes of derangement, which appear to be more or less concerned in different kinds of fever, there are other more partial ones, which seem to have a similar relation to local inflammations. The most obvious of these changes are such as must be produced by partial dilatations or contractions of the capillary vessels; since, as I have endeavoured to demonstrate, any supposed derangement in the actions of the larger vessels must be excluded from the number of causes which can materially affect the circulation. It cannot be denied, that a diminution of the elastic, or even of the muscular force of the small arteries, must be immediately followed by such a distension, as will produce a resistance equal to the pressure: the distension will occasion an increase of redness, and in most cases pain: the heat will also generally be increased, on account of the increased quantity of blood, which will be allowed to pass through the part; and since the hydrostatic pressure of the blood acquires greater force, as the artery becomes more distended, it may be so weak as to continue to give way, like a ligament which has been strained, until supported by the surrounding parts. In this state a larger supply of blood will be ready for any purposes which require it, whether an injury is to be repaired, or a new substance formed; and it is not impossible, that this change in the state of the minute vessels may ultimately produce some change in the properties of the blood itself.

Partial affections of the capillary vessels.

Inflammation.

Perhaps the properties of the blood ultimately altered.

The more the capillary arteries are debilitated and distended, the greater will be the mean velocity of the circulation; but whether or no the velocity will be increased in the vessels which are thus distended, must depend on the extent of the affected part; and it may frequently happen, that the velocity may be much more diminished on account of the dilatation of the space which the blood is to occupy, than increased by the diminution of the resistance. And on the other hand, the velocity may be often increased, for a similar reason, at the place of a partial contraction. Hence we may easily understand some of the experiments, which Dr. Wilson has related in his valuable treatise on fevers: the

Experiments in Wilson on Fevers.

the application of spirit of wine to a part of the membrane of a frog's foot contracted the capillary arteries, and at the same time accelerated the motion of the blood in them, while in other parts, where inflammation was present, and the vessels were distended, the motion of the blood was slower than usual.

Another species of inflammation.

Another species of inflammation may probably be occasioned by a partial constriction or obstruction of the capillary arteries, which must indeed be supposed to exist where the blood has become wholly stagnant, as Dr. Wilson in some instances found it. This obstruction must however be extended to almost all the branches, belonging to some small trunk, in which the pressure remains nearly equal to the tension of the large arteries; for in this case it will happen, that the whole pressure will be continued throughout the obstructed branches, without the subtraction of the most considerable part, which is usually expended in overcoming the resistances dependent on the velocity; so that the small branches will be subjected to a pressure, many times greater than that which they are intended to withstand in the natural state of the circulation; whence it may easily happen, that they may be morbidly distended; and this distension may constitute an inflammation, attended by redness and pain. Nor is it impossible, that obstructions of this kind may originate in a vitiated state of the blood itself, although it would be difficult to prove the truth of the conjecture; it seems, however, to be favoured by the observation of Haller, that little clots of globules may often be observed in the arteries, when the circulation is languid, and that they disappear when its vigour is restored, especially after venesection. But if a very small number only of capillary arteries be obstructed, other minute branches will still be capable of receiving the blood, which ought to pass through them, without any great distension or increase of pressure; and this exception is sufficient to explain another experiment of Dr. Wilson, in which a small obstruction, caused by puncturing a membrane with a hot needle, failed to excite an inflammation. This species of inflammation is probably attended by less heat than the former; and where the obstruction is very great, it may perhaps

perhaps lead immediately to a mortification, which is called by the Germans "a cold burning."

The most usual causes of inflammation appear to be easily reconcilable with these conjectures. Suppose any considerable part of the body to be affected by cold; the capillary vessels will be contracted, and at the same time the temperature of some parts of their contents will be lowered, from both of which causes the resistance will be increased, and the arteries in general will be more or less overcharged: if then any other part of the system be at the same time debilitated or overheated, its arteries will be liable to be morbidly distended, and an inflammation may thus arise, which may continue till the minute vessels are supported and strengthened, by means of an effusion of coagulable lymph. The immediate effect, either of cold or of heat, may also sometimes produce such a degree of debility in any part, as may lay the foundation of a subsequent inflammation: but the first effect of heat in the blood-vessels appears to be the more ready transmission of the blood into the veins, by means of which they become very observably prominent: and cold, which checks the circulation in the cutaneous vessels, probably occasions a livid hue, by retaining the blood stagnant longer than usual in the capillary vessels of all kinds. It may be objected, that an obstruction of the motion of the blood through a great artery ought, upon these principles, to produce an inflammation in some distant part: but in this case, the blood will still find its way very copiously into the parts supplied by the artery, by means of some collateral branches, which will always admit a much larger quantity of blood than usually passes through them, whenever a very slight excess of force can be found to carry it on, or when the blood which they contain finds a readier passage than usual, by means of their communication with such parts as are now deprived of their natural supply.

These conjectures consistent with the common causes of inflammation.

It is difficult to determine, whether blushing is more probably effected by a constriction or by a relaxation of the vessels concerned; it must, however, be chiefly an affection of the smaller vessels, since the larger ones do not contain a sufficient quantity of blood to produce so sudden an effect.

Blushing.

Perhaps

Perhaps the capillary vessels are dilated, while the arteries, which are a little larger only, are contracted: possibly too an obstruction may exist at the point of junction of the arteries with the veins; and where the blush is preceded by paleness, such an obstruction is probably the principal cause of the whole affection.

Tendency of inflammation to extend itself.

With respect to the tendency of inflammation in general to extend itself to the neighbouring parts, it is scarcely possible to form any reasonable conjecture, that can lead to its explanation: this circumstance appears to be placed beyond the reach of any mechanical theory, and to belong rather to some mutual communication of the functions of the nervous system; since it is not inflammation only, that is thus propagated, but a variety of other local affections of a specific nature, which are usually complicated with inflammation, although they may perhaps, in some cases, be independent of it. Inflammations, however, are certainly capable of great diversity in their nature, and it is not to be expected, that any mechanical theory can do more than to afford a probable explanation of the most material circumstances, which are common to all the different species.

Operation of remedies for inflammation and fevers.

Beside these general illustrations of the nature of fevers and inflammations, the theory which has been explained may sometimes be of use, in enabling us to understand the operation of the remedies employed for relieving them. Thus it may be shown, that any diminution of the tension of the arterial system must be propagated from the point at which it begins, as from a centre, nearly in the same manner, and with the same velocity, as an increase of tension, or a pulsation of any kind would be propagated. Hence the effect of venesection must be not only more rapidly, but also more powerfully felt in a neighbouring than in a distant part: and although the mean or permanent tension of the vessels of any part must be the same, from whatever vein the blood may have been drawn, provided that they undergo no local alteration, yet the temporary change, produced by opening a vein in their neighbourhood, may have relieved them so effectually from an excess of pressure, as to allow them to recover their natural tone, which they could not have done without such a partial exhaustion of the

Topical venesection.

the neighbouring vessels. But since it seems probable, that the minute arteries are more affected by distension than the veins, there is reason in general to expect a more speedy and efficacious relief in inflammations, from opening an artery than a vein: this operation, however, can seldom be performed without material inconvenience; but it is probably for a similar reason, that greater benefit is often experienced from withdrawing a small portion of blood by means of cupping or of leeches, than a much larger quantity by venesection, since both the former modes of bleeding tend to relieve the arteries, as immediately as the veins, from that distension, which appears to constitute the most essential characteristic of inflammation. In a case of hemorrhage from one of the sinuses of the brain, a very judicious physician lately prescribed the digitalis: if the effect of this medicine tends principally to diminish the action of the heart, as is commonly supposed, it was more likely to be injurious than beneficial, since a venous plethora must be increased by the inactivity of the heart; but if the digitalis diminishes the general tension of the arteries, in a greater proportion than it affects the motion of the heart, it may possibly be advantageous in venous hemorrhages. We have, however, no sufficient authority for believing, that it has any such effect on the arterial system in general.

Arteriotomy.

Cupping and leeches.

Use of foxglove in hemorrhage.

Although the arguments, which I have advanced, appear to me sufficient to prove, that, in the ordinary state of the circulation, the muscular powers of the arteries have very little effect in propelling the blood, yet I neither expect nor desire, that the prevailing opinion should at once be universally abandoned. I wish, however, to protest once more against a hasty rejection of my theory, from a superficial consideration of cases, like that which has been related by Dr. Clarke; and to observe again, that the objections, which I have adduced, against the operation of the muscular powers of the arteries in the ordinary circulation, not being applicable to these cases, they are by no means weakened by any inferences which can be drawn from them.

Muscular powers of the arteries have little effect on the ordinary circulation.

IV.

IV.

Description of a Scarificator on a new Principle. By Mr. THOMAS SHUTE, *Surgeon.*

SIR,

New scarificator.

IF the annexed description of a Scarificator, which I have found upon trial to be extremely efficient, should appear worthy of insertion in your Journal, I have taken the liberty of transmitting it to you for that purpose.

I am, Sir,

Your most obedient humble Servant,

Park Street, Bristol, THOMAS SHUTE, *Surgeon.*
22d July, 1810.

Cupping evidently advantageous.

The advantages resulting from a local evacuation of blood by cupping, in a variety of complaints, being fully established, it would, I presume, be a waste of time elaborately to descant on the merits of such depletion, as forming a high and important remedy in the curative art. It must however be admitted, that the operative means, which have been hitherto employed for this purpose, are not only too often tedious and painful in their applications, but very frequently extremely ineffectual in the event. Such being the acknowledged fact, and regarding it as very improbable, that the difficulty of obtaining blood could depend on a want of manual dexterity in the operator, when the scarificator usually employed had passed through the hands of so many able practitioners, it seemed natural to conclude, that the want of success ought rather to be attributed to some fault in the construction of the instrument itself. Impressed with these ideas, and having taken up an opinion, that the failure of the scarificator now in use might be attributed to the manner in which the incisions are made; and supposing, that simple punctures would more certainly enter the depths intended; I flatter myself, that, by altering the principle on which the instrument used to act, I have produced one, which will effect all the purposes required with more facility to the operater, and less pain to the patient.

The operation sometimes tedious, painful, and ineffectual,

probably from defects of the instrument.

Alteration of its principle.

Without

Without any intention then extravagantly to extol the properties of a new instrument, or unnecessarily to depreciate the merits of an old one, I take the liberty of recommending one to my medical brethren for their approbation, which I have found to answer in my hands much better than any other that I have yet been able to procure. That the instrument here recommended will invariably produce the wished for effect, I am sanguine enough to believe; at the same time that I, by no means, mean to assert it is not still capable of farther improvement. This instrument effectual.

A draught taken by Mr. Mac Donald, a friend and pupil of mine, is subjoined, sufficiently explanatory as I hope of the fabric of the instrument, which may be purchased of Mr. Winter, Cutler, Bridge Street.

It is my intention, at no very distant period, to offer a few observations on the formation and number of the lancets, so as more immediately to adapt them to particular parts of the body.

Explanation of the Plate.

Plate IV, fig. 1, 2, and 3. *a* a nut, by means of which, acting on the screw *b*, the plates *c* and *d*, holding the lancets, are drawn upwards, till the catch *e* falls into the notch at *f*. The nut is then unscrewed; and, by pushing in the knob *g*, the catch is withdrawn, and the worm spring *h* immediately forces down the lancets. The scarificator described.

i, a spring acting on the catch *e*, to force it into the notch.

k, a box, which, by means of the screws *l l*, regulates at will the exposure of the lancets, and in consequence the depth of the incisions.

The figures are on a scale of half an inch to an inch.

V.

On the Theory of Capillary Attraction. By THOMAS KNIGHT, *Esq.* In a Letter from the author.

To Mr. NICHOLSON.

SIR,

Theory of capillary action imperfect.

NOTWITHSTANDING the attention, that has lately been paid to the phenomena arising from capillary action, the theory appears to be still in a very imperfect state. I have endeavoured, in what follows, to throw some light on that part, which is most defective. The insertion of it, in your valuable Journal, will speedily bring my ideas before competent judges,

And much oblige, Sir,

Your most obedient Servant,

Papcastle, near Cockermouth, THOMAS KNIGHT.
July 22d, 1810.

The phenomena may be estimated from the figure of the surface, or from the forces.

There are two ways of treating the subject of capillary action; first, the measure of the *other* phenomena may be estimated from the figure of the surface, which is a *simultaneous effect* with the height of the fluid: or, secondly, in a more natural manner, from the forces themselves which support the fluid.

Mistake of Mr. la Place.

If, with Mr. La Place and Dr. Young, the first method be made use of, we must take care not to mistake an effect for the cause, as the former of these authors appears *at first* to have done*. But, even if we do not fall into this errour, a theory, which stops here, must appear, I think, to any one, to be exceedingly defective. Let the hydrostatic principle, of the perpendicularity of the force to the surface, be used to explain what relates to the figure of that surface:

* It is very remarkable, that Dr. Young, in his observations on Mr. la Place's theory, should not have noticed the chief circumstance in which it differs from all others: viz. "Que l'eau s'élève, dans un tube capillaire, *par l'effet* de la concavité de sa surface intérieure." Sur l'Action capillaire p. 60. Whether Dr. Young himself be of this opinion, I do not very clearly perceive.

but,

but, by all means, let us have a view of *the mechanism by which the whole column is supported.*

Mr. La Place, probably from considering the matter in this light, gave the world his *second* method; which appears to be as erroneous in falsely explaining the true*, as the *first* had been in assigning a wrong cause. Errour in his second method.

My own intention, at present, is to show what part of a capillary tube keeps a fluid elevated; and the precise manner, in which it causes this elevation: or, in other words, to supersede this second method of Mr. La Place, and all similar theories, by one more conformable to truth. Object of the author.

The remarkable experiment of Abat, which Mr. La Place seems to have thought one of the best proofs of his first theory, will be very easily explained here on quite different principles. Experiment of Abat.

We shall also see what is *the limit* of the height of the fluid in that experiment, which no one I believe has yet shown.

I suppose, with Mr. La Place, and other writers on the subject, that the attractions of the particles of the fluid for itself, and of the tube for the fluid, extend only to insensible distances; that they follow the same law of the distances; and only differ by their intensity at the same distance. Attraction of the fluid for itself and for the tube differs only in intensity.

Prop.

Let A B C D E F (fig. 4) be the section, through the axis, of a circular tube, every where of equal diameter, bent into a rectangular form, and standing in a vertical plane. Let the part A B C D, of the tube, be formed of matter, the intensity of attraction of which for the fluid within it is represented by r, while that of the other part C D E F is r'. The excess of the mass of fluid in the leg A B over that in the leg E F is as $(2r-2r')$ × diameter of the tube. Proposition.

Let $e\ u\ m\ n\ s$ be a slender canal of fluid, extending from the surface in one leg to that in the other, and parallel to the side of the tube, as well as every where at the same distance from it. It is, in the first place, evident that this Demonstration.

* The balancing force of the tube.

canal

canal is not urged either way, except by those parts of the tube which are situate near the surface of the fluid.

For, from l, any particle of the tube, set off $l\,m$, $l\,n$, equal to one another, and of any length less than the radius of the sphere of action of the particle.

If this particle urges the canal in one direction by its action at m, it urges it equally in the contrary direction by its action at n*.

We will now see what action the canal sustains near the surface $c\,e\,d$. With a radius $e\,f$, equal to that of the sphere of action of the particles, and with the point e for a centre, describe the circular arc $o\,f\,p$.

The canal $e\,r$ is urged upwards by the resolved action of those particles of the section of the tube contained in the space $o\,f\,p\,d\,o$; and if this space be divided into two equal parts, by the horizontal line $e\,f$, the action of the part above this line draws the canal as much upwards as that of the lower part does. For from any point g, in the lower part, draw $g\,e$, $g\,h$ equal to one another; the action of g on the part $e\,h$ urges the canal neither upwards nor downwards; for its action on any point above h is counteracted by its action on another point at the same distance below e. But there are particles below h, and within the sphere of action of g, as at k, on which it exercises an unbalanced action tending to draw the canal upwards. Next, suppose i, in the upper part of the space $o\,f\,p\,d\,o$, similarly situate to g in the lower. Draw $e\,i$, which will be parallel to $g\,h$, and will evidently draw upwards that part of the canal below e, as much as g draws upwards the part below h.

The other end $s\,\varrho$, of the canal $e\,n\,m\,s$, is urged upwards,

* This is so plain, that one is astonished to find Mr. la Place, in his second method, making the chief part of the force, which elevates the fluid, reside at the junction of the two tubes. See "*Supplément a la Théorie de l'Action Capillaire*," p. 16. Clairaut also, in a theory, to which it has been lately the fashion to give very undeserved praise, falls into the same errour. "*Figure de la Terre*," p. 119. The false proposition, that a mass with a plane surface presses a slender column within it *downwards*, to which most of Mr. la Place's errours may be traced, has Clairaut for its original author. Haüy has the same figure, and nearly the same words.

in

in like manner, by the action of the space $\omega \varphi \pi \delta \omega$, equal to, and described in the same manner as, $o f p d o$.

Now, suppose both sides of the section of the tube to revolve round the axes $a b$, $\alpha \beta$; the cylindrical annulus generated by $e r$ is urged upwards by the action of the annulus generated by the circular space $o f p d o$; and that generated by εg by that generated by $\omega \varphi \pi \delta \omega$. If we represent the action of the annulus generated by $o f p d o$ by $2 r$, that of the annulus generated by $\varpi \varphi \pi \delta \omega$ will be $2 r'$.

By reasoning in the same manner with respect to all other cylindrical annuli within the sphere of action of the tube, and taking the sum of all the actions, it is plain, that the excess of the mass of the fluid in the leg A B over that in the leg E F is as $2 r - 2 r'$.

But now, supposing all other things to remain the same, conceive the diameter of the tube to vary. A canal $e r$, at the same distance from the side, in tubes of different diameters, will be equally attracted by all of them: for that part of the surface of the tube, which attracts the canal, is so small (by the hypothesis) that it may be considered as plane whatever be the tube's diameter.

Therefore, while the diameter of the tube varies, as the number of equal columns in a cylindrical annulus of given breadth, at a given distance from the tube, but within the reach of its action, varies *quam proxime* as the diameter of the tube, while the force urging upwards each separate column is constant, it is easy to see, by collecting as before the sum of the forces acting on different annuli, that the excess of the mass in the leg A B over that in E F is as the diameter of the tube.

By combining both parts of the proposition, and supposing the diameter of the tube, and the attractions of its two ends to vary together, the excess of the mass in A B over that in E F will be as $(2 r - 2 r') \times$ diameter. Q E D.

Cor. 1st. It is plain from the above demonstration, that the mass of fluid, supported by a capillary tube, depends not in the smallest degree on the figure $c a d$ of its surface, so that, *if it were possible for us to make this surface take any other conceivable figure, the same mass of fluid would* Corollary 1.

still be supported. The figure of the surface *is* a *secondary effect*, exactly in the same manner as the catenarian form of a supported chain is a secondary effect; the cause being the pegs and the force of gravity.

When, however, I say, that the figure of the surface is of no consequence, I suppose that surface to be at a sensible distance below the orifice of the tube, otherwise the case will be very different: for,

Corollary 2. Cor. 2d, Suppose, in fig. 5, every thing to remain as before, excepting that the whole tube is made of one kind of matter, (the intensity of attraction for the fluid within it being r) and that the left hand branch terminates at $\gamma\delta$, close to the surface of the fluid. I say that the difference of masses, in the two branches, will, in great measure, depend on the figure of *that part of the surface of the fluid*, at the orifice $\gamma\delta$, *which is near to* γ *and* δ: and that we should have the greatest difference of masses if it were possible to make the fluid, at this orifice, take any figure as $\delta x z \gamma$ *, perpendicular at the sides, and having every point of its upper surface distant from γ and δ by a space greater than the radius of the sphere of action of the particles. And the difference of masses in this case, would be to the difference of masses when the surface is horizontal, as $\gamma\varepsilon\delta$, at the orifice of the tube, as 2 to 1.

For, in this latter case, when the surface of the fluid is $\gamma\varepsilon\delta$, it is evident, from the reasoning used in the proposition, that the left hand mass is drawn upwards by a force as r, and the right hand mass by a force as $2r$; whence the difference of the masses is as r. But, if the left hand mass could stand at xz, and the column $\varepsilon\rho$ become $y\rho$, any such column, and consequently the whole left hand mass, would be urged neither upwards nor downwards by the branch $\gamma\delta$ of the tube: therefore the difference of masses would be entirely occasioned by the other branch, and would be as $2r$.

Now, though we cannot make the fluid stand at xz, we

* Mr. Haüy, explaining Abat's experiments after the ideas of la Place, falls, in consequence, into an errour. See 'Traité de Physique,' tom. 1, p. 243, Ed. 2. 'le petit changement de figure lui donne *plus de force*,' &c.

may

may give it the convex form $\delta \varepsilon \gamma$; and if $c\ d$ was the surface in the branch A B, when that in the other branch was $\delta \varepsilon \gamma$; let this last become convex, as $\delta \varepsilon \gamma$, and the first shall be $\omega \pi$; the difference of masses, in the latter case, being to that in the former in a ratio approaching the more nearly to that of 2 to 1, the higher we can make the convex surface $\delta \varepsilon \gamma$, and the nearer its sides at γ and δ are to perpendicularity with $\delta \varepsilon \gamma$.

Here then is the *true* and simple explanation of Abat's experiment, which in no respect depends on the figure of the surface, *in the ſenſe that Mr. la Place means**.

Cor. 3rd. Let A B $p\ r$, fig. 6, represent a tube of such intensity of attraction, that, if it be immersed in a fluid the horizontal surface of which is $f\ c\ d$, this surface shall undergo no alteration. Corollary 3.

Suppose this tube cut off close to the surface; as, by this, the part $o f c$ (a quadrant to the radius of the sphere of attraction) to which half its effect is owing (by the proposition) is taken away, the intensity of attraction of the lower part $f c p$ must be doubled to preserve the equilibrium: and it plainly follows, that the intensity of attraction of the fluid for itself is twice that which the tube before it was cut off had for the fluid.

May I not say, that so *peculiar* a demonstration, of a theorem easily proved in other ways, is of itself sufficient to establish the truth of this theory?

Cor. 4th. If, in fig. 4, we suppose the branch E F to be cut off close to the surface (which I suppose horizontal) and then to be of the same intensity of attraction with the Corollary 4.

* After the same manner is explained another experiment mentioned by Mr. la Place, 1st. supplement, p. 60. Plunge a capillary tube into water, and, having closed the lower orifice with the finger, draw it out of the water. If we now remove the finger, the fluid will fall in the tube, and form a convex drop at the lower orifice. But, when it has ceased to descend, the height of the column always remains greater than the height of the water in the tube, above the level, when it was plunged in the fluid. "*This excess* (says Mr. la Place) *is owing to the action of the drop of water on the column.*"

The *true* explanation is the same as that I have given above of Abat's experiment.

fluid it contains, we have the case of a common capillary tube; and the mass raised above the level of the surface of the fluid in the vessel is as $(2r - r') \times$ diameter of the tube. From this corollary the explanation of the common phenomena is too simple to make it necessary for me to dwell on it.

VI.

An Account of the Effects of Thirty Tons of Quicksilver escaping by the rotting of leathern Bags into the Bilge Water, on board the Triumph Man of War: communicated by Dr. BAIRD, *Physician General to the Navy, to a Friend in London.*

Quicksilver taken on board a ship

IN April, 1810, the Triumph man of war took on board thirty tons of quicksilver, contained in leathern bags of 50lbs. each. These bags were picked up on the shore of Cadiz, from the wreck of two Spanish line of battle ships, lost in the storm immediately preceding, at the end of March, the above date. The collected bags were stowed below, in the bread room, after hold, and store rooms: they were saturated with salt water, and, in about a fortnight, all decayed and burst. In the act of collecting and endeavouring to save the quicksilver in casks, much of it found its way to the recesses of the ship, beyond the possibility of recovery. Some portion, however, was secreted by the men, who amused themselves in various ways with it, as cleaning their spoons, &c.

in wet leathern bags, which burst.

The vapour of the bilge water whitened metals, instead of blackening them,

At this period bilge water had collected in the ship, the stench from which was intolerable; and the carpenter's mate, in the act of sounding the well, was nearly suffocated. The effect of the gas escaping from bilge water is manifested, by its changing every metallic substance in the ship black. But in this instance metals of every kind were coated with quicksilver; and a general affection of the mouth took place among the men and officers, to a severe

degree

degree of ptyalism, in upwards of 200 men. The ship was sent to Gibraltar, had all her stores taken out, the hold made clean, and all the quicksilver, that could be reached, removed; but near ten tons are supposed still to remain between the ship's timbers below, which can only be cleared away by docking the ship, and dislodging a plank at the most descending part near the keel. Since the process of cleaning the ship has taken place, and a new atmosphere created, all effects from the quicksilver have ceased.

and salivated great part of the crew.

Dr. Baird having requested an explanation from his friend in London, the following account was transmitted to Plymouth.

To Dr. BAIRD.

From well established principles, as well as analogies, a very reasonable explanation may be given of the effects attributed to thirty tons of quicksilver, exposed on board the Triumph in bilge water, with rotten leathern bags, in a hot climate, the beginning of summer.

This accounted for.

The stinking gas, which was generated, was sulphuretted and perhaps phosphuretted hidrogen gas, mixed with carbonic acid and perhaps other gasses compounded by the putrefaction of animal and vegetable matter. The deadly suffocating effects of which gasses are fully ascertained, unless diluted with a large proportion of fresh air; and the tarnishing of metals, especially of silver, at a great distance, even when mixed with a large proportion of fresh air, is a well known effect of sulphuretted hidrogen.

Gasses naturally arising from bilge water.

These last named effects are attributable to the gasses of putrefaction independently of quicksilver. But when the influence of so large a body of this metal is considered, it will be easy to account for the whitening of metals, and the salivation or sore mouth of many persons in the ship. The quicksilver would rise united or suspended by the above gasses, or be even evaporated by the heat of the ship, in the common fresh air. This metal so suspended or dissolved is very likely to penetrate the human body, and act upon it like the fumigation with quicksilver; but sulphuretted

The quicksilver would rise in part from the action of heat,

and likewise from being dissolved by the sulphuretted hidrogen.

etted hidrogen dissolves the metal, and of course would carry it wherever the gas was transmitted.

It will be understood, that the sulphur and phosphorus are furnished probably by the decomposition of sulphuric and phosphoric acids, always present in all kinds of animal matter. The hidrogen gas is furnished chiefly by the decomposition of the water. The carbonic acid is compounded by the union of the charcoal of the animal and vegetable matter with the oxigen (principally) of the water.

Sources of the several gasses,

and of the stench.

The stink is now imputed to the mixture of sulphuretted and phosphuretted gasses with putrifying matter. If the leathern bags of quicksilver had been kept dry, they would not have putrefied, but probably would have retained the metal, and the above effects would not have happened.

VII.

Scheme for preserving the Lives of Persons Shipwrecked. By G. CUMBERLAND, *Esq.*

SIR,

Hints for preserving persons shipwrecked.

ENCOURAGED by your ready insertion of such papers as I have addressed to you at my leisure moments, that I thought may be of use to society; I take the liberty to propose the publication of some crude reflections on a subject of very great national importance: and although once presented to the Admiralty * without producing even an acknowledgment, I feel, that you will not think such an idea fit to be totally rejected; as, if not immediately put into practice, it may, by being recorded, be the means of ultimately producing, from better heads, some improved provisions, that shall render naval services less dangerous to those, who are the support, the defence, and the bulwark of the nation.

* My letter was addressed to Lord Melville, then first Lord of the Admiralty.

The

The grandson of the man who first invented the bending of ship timber by means of hot sand, in the very cases, which now are filled with boiling water; who ruined himself by expending £16,000 to enrich his country, was rewarded with a delusive patent, and left his children in want; may be allowed to be disinterested in any proposal he makes for the benefit of a navy, that, as individuals, has only been to them productive of disappointment and irretrievable loss.

Invention of bending ship timber ruinous to the projector.

About six years past, a solitary inhabitant of a promontory projecting into the Severn Sea, called Weston Super Mare, I amused myself much among the rocks there, and spent many hours in studying the action and form of water when impelled in the figure of a wave; it being my opinion at that time, as it still is, that the forms water takes from motion are so determined, that even in sculpture they may be represented with correctness; and that nothing would better teach us the art of representing motion by fixed lines, than these images so often repeated with exactness. On these occasions I frequently observed extensive masses of the sea weed called tang on that coast, and which the farmers burn for manure, floating into the hollow coves below me, on the surface of the most tremendous waves; and forming, if I may so express myself, a green carpet, that, undulating on the broken wave, was never submerged, although continually varying its surface; and on which, as on a resting place, birds frequently alighted, or sat to repose themselves, as if it were a verdant down.

Natural phenomenon that suggested the idea to the author.

On a coast so remarkably dangerous, where no boat could land even in comparatively tranquil weather, these *safe rafts* were very interesting, and led naturally to the thought, whether such a sort of raft might not be constructed of other materials, fit, instead of birds, to carry men. The result of which was, it appeared to me, that if each sailor in a man of war had a cork mattress, and these mattresses were all linked together by cords, such a float, capable of landing safely even on breakers, would be produced.

Its practical application.

Pleased with the thought I went to Bristol, and consulted a cork cutter there as to the quantity of cork necessary to support a man; and soon found, that a very moderate weight would do, and that cork shavings were then worth only

Cork shavings would make cheap & good mattresses, and effectual for the purpose.

only 8*d.* per bushel, and chiefly sold for firing, or to make guards for privateers to fill the nettings.

It therefore struck me, that, as mattresses are necessary in the navy for the hammocks, and nothing dryer than cork or easier to shave into a thin elastic body, it might answer the above end, to fill these mattresses with this substance, in a proportion equal to the support of a single man: and then a mass of them thrown overboard linked together by ties at each corner, where cords might be always attached, would form an extensive raft, capable of sustaining, out of the water, as many men as there were of these mattresses united; and thus conveying them on the tops of the waves, and depositing them safely on shore, or even on the surface of rocks, when the sea retired with the tide.

This plan suggested to the Admiralty.

To contemplate such a thought in imagination is truly delightful; but to believe, as I do, that the thing is practicable with ease, and not communicate it to others, is impossible. I have therefore done all in my power to extend the idea from my own bosom to the mind of the public at large, having first addressed my wishes and plan to that quarter, where the power of putting it extensively into execution alone exists.

As your Journal must ultimately reach all countries, I therefore wish to deposit these reflections in it, in the hope, that they may thus be extended to some practicable benefit, if not to ourselves, to our neighbours, or some distant clime, where the coasts are equally dangerous: for all other rafts, that I have either seen or contemplated, have this great defect, that they come on shore with too much force, and that the blows they receive either disjoint them, or throw off the people; that their wrecks are more dangerous than the rocks they strand on; and that every time they pitch those on them are covered, and some never may be able to retain their hold or rise again.

Defects of all other rafts.

I am, &c.

Bristol, 17th Aug. 1810. G. CUMBERLAND.

VIII.

VIII.

Method of ascertaining the Value of Growing Timber Trees, at different and distant Periods of Time. By Mr. CHARLES WAISTELL, *of High Holborn.*

(Continued from p. 31.)

A TABLE *showing the Number of Trees to be cut out in thinning of Woods, and the Number left standing at every Period of 4 Years from 20 up to 64 Years.* Table 3. For thinning woods.

IN the 24th volume of the Transactions of the Society of Arts, &c., page 75*, Mr. Salmon, in a paper on the Management of Fir Woods, says, "the distance of trees from each other should be one fifth of their height." At this distance, which is probably sufficient for fir trees, the following will be the number on an acre, and the number to be cut out at the ages and heights under-mentioned, and the number of feet they will then contain in the bole, when measured to the top of the leading shoot. These trees are supposed to increase twelve inches in height, and one in circumference annually, and to have been at first planted four feet apart.

TABLE III.

Years old and feet high.	Girt.	Contents			Distance	Number of Trees on an Acre.	Contents of the whole.	Number to be cut out.	Contents.
	inch.	ft.	in.	pts.	feet.		feet.		feet.
20	2¼	0	10	5	4·	2722	2362	839	727
24	3	1	6	0	4·8	1883	2824	494	741
28	3¼	2	4	7	5·6	1389	3308	326	776
32	4	3	6	8	6.4	1063	3779	223	792
36	4¼	5	0	9	7·2	840	4252	160	810
40	5	6	11	4	8·	680	4722	118	819
44	5¼	9	2	11	8·8	562	5194	90	831
48	6	12	0	0	9·6	472	5664	70	840
52	6¼	15	3	0	10·4	402	6130	55	838
56	7	19	0	8	11·2	347	6611	45	857
60	7¼	23	5	2	12.	302	7076	37	866
64	8	28	5	4	12·8	265	7537		

* See Journal, vol. XVII, p. 162.

And

Table 4. For thinning woods.

And if trees be periodically thinned out to the distance of one fifth of their height, and that they increase fifteen inches in height, and one inch and a half in circumference annually, the number of trees on an acre, and the number to be cut out at different periods, and the number of feet they will respectively contain at those periods, will be as under, viz.

TABLE IV.

Age.	Height.	Girt.	Contents,			Distance.	Number of trees on an acre.	Contents of the whole.	Number to be cut out.	Contents.
years.	feet.	inch.	ft	in.	pts.	feet.		feet.		feet.
16	20	3	1	3	0	4	2722	3402	980	1225
20	25	3¾	2	5	3	5	1742	4246	532	1296
24	30	4½	4	2	7	6	1210	5100	322	1357
28	35	5¼	6	8	4	7	888	5944	208	1392
32	40	6	10	0	0	8	680	6800	143	1430
36	45	6¾	14	2	10	9	537	7644	102	1452
40	50	7½	19	6	4	10	435	8494	75	1464
44	55	8¼	25	11	10	11	360	9355	58	1507
48	60	9	33	9	0	12	302	10192	45	1518
52	65	9¾	42	10	10	13	257	11026	35	1501
56	70	10½	53	7	0	14	222	11895	29	1553
60	75	11¼	65	10	11	15	193	12720	23	1515
64	80	12	80	0	0	16	170	13600		

Remarks.

It will be observed in all these tables, that when trees have doubled their age, there are only one fourth of the number remaining on an acre, in consequence of their distance being doubled; but as each tree will then have increased its contents eight-fold, therefore the number of feet on an acre must be then doubled. Above, at 64 years of age, there is exactly double the number of feet that there is at 32 years of age.

Table 5. for thinning woods.

And if trees be periodically thinned out to the distance of one fifth of their height, and that they increase eighteen inches in height, and two inches in circumference, annually, the

the number of trees on an acre, and the number to be cut out at different periods, and the number of feet they will then respectively contain, will be as under, viz.

TABLE V.

Age.	Height.	Girt.	Contents.			Distance.	Number of trees on an acre.	Contents of the whole.	Number to be cut out.	Contents.
years	feet.	inch.	ft.	in.	pt.	feet.		feet.		feet.
12	18	3	1	1	6	4·	2722	3062	839	943
16	24	4	2	8	0	4·8	1883	5021	673	1794
20	30	5	5	2	6	6·	1210	6302	370	1927
24	36	6	9	0	0	7·2	840	7560	223	2007
28	42	7	14	3	6	8·4	617	8817	145	2072
32	48	8	21	4	0	9·6	472	10069	99	2112
36	54	9	30	4	6	10·8	373	11314	71	2153
40	60	10	41	8	0	12·	302	12583	52	2166
44	66	11	55	5	6	13·2	250	13864	40	2213
48	72	12	72	0	0	14·4	210	15120	32	2304
52	78	13	91	6	6	15·6	178	16294	24	2197
56	84	14	114	4	0	16·8	154	17607	20	2286
60	90	15	140	7	6	18·	134	18843	16	2250
64	96	16	170	8	0	19·2	118	20138		

Table 6. for thinning woods.

But if the trees be first planted four feet apart, and be periodically thinned out to the distance of one fourth of their height, and they increase twelve inches in height, and one in circumference annually, the number of trees on an acre, and the number to be cut out at the ages and heights under-mentioned, and the number of feet they will respectively contain in the bole, when measured to the top of the leading shoot, will be as under, viz.

TABLE

TABLE VI.

Years old & feet high.	Girt.	Contents.			Distance	Number of trees on an acre.	Contents of the whole.	Number to be cut out.	Contents.
	inch.	ft.	in.	pt.	feet.		feet.		feet.
16	2	0	5	4	4	2722	1209	980	435
20	2½	0	10	5	5	1742	1512	532	461
24	3	1	6	0	6	1210	1815	322	483
28	3½	2	4	7	7	888	2115	208	495
32	4	3	6	8	8	680	2417	143	508
36	4½	5	0	9	9	537	2718	102	516
40	5	6	11	4	10	435	3020	75	520
44	5½	9	2	11	11	360	3327	58	536
48	6	12	0	0	12	302	3624	45	540
52	6½	15	3	0	13	257	3919	35	533
56	7	19	0	8	14	222	4230	29	551
60	7½	23	5	2	15	193	4522	23	538
64	8	28	5	4	16	170	4835	20	568
38	8½	34	1	4	17	150	5116	16	545
72	9	40	6	0	18	134	5427	14	567
76	9½	47	7	6	19	120	5715	12	571
80	10	55	6	8	20	108	6000	10	555
84	10½	64	3	8	21	98	6301	8	554
88	11	73	11	4	22	90	6655	8	591
92	11½	84	5	11	23	82	6928	7	591
96	12	96	0	0	24	75	7200	6	576
100	12½	108	6	0	25	69	7486	5	542
104	13	122	0	8	26	64	7811	5	610
108	13½	136	8	3	27	59	8037	4	546
112	14	152	5	4	28	55	8384	4	609
116	14½	169	4	5	29	51	8659	3	508
120	15	187	6	0	30	48	9000	3	562
124	15½	206	10	7	31	45	9309	3	620
128	16	227	6	8	32	42	9557	2	455
133	16½	249	6	8	33	40	9882		

And

And if the trees be periodically thinned out to the distance of one fourth of their height, and they increase 15 inches in height, and one inch and a half in circumference annually, the number of trees on an acre, and the number to be cut out at the different periods undermentioned, and the number of feet they will respectively contain at those periods, will be as under, viz. Table 7 for thinning woods.

TABLE VII.

Age.	Height.	Girt.	Contents.			Distance	Numb of trees on an acre.	Contents of the whole.	Number to be cut out.	Contents.
Years	feet.	inch.	feet.	in.	pts.	feet.		feet.		feet.
12	15	2¼	0	6	3	4·	2722	1417	980	510
16	20	3	1	3	0	5·	1742	2177	627	783
20	25	3¾	2	5	3	6·25	1115	2717	341	831
24	30	4½	4	2	7	7·5	774	3262	206	868
28	35	5¼	6	8	4	8·75	568	3802	133	890
32	40	6	10	0	0	10	435	4350	91	910
36	45	6¾	14	2	10	11·25	344	4897	66	938
40	50	7½	19	6	4	12·5	278	5428	48	937
44	55	8¼	25	11	10	13·75	230	5976	37	962
48	60	9	33	9	0	15·	193	6513	29	978
52	65	9¾	42	10	10	16·25	164	7036	22	943
56	70	10½	53	7	0	17·5	142	7608	19	1018
60	75	11¼	65	10	11	18·75	123	8106	15	988
64	80	12	80	0	0	20·	108	8640		

And if the trees be planted at 4½ feet apart, and be periodically thinned out to the distance of one fourth of their height, and they increase 18 inches in height and 2 inches in circumference annually, the number of trees on an acre, and the number to be cut out at the different periods undermentioned, and the number of feet they will then respectively contain, will be as under, viz. Table 8 for thinning woods.

TABLE

TABLE VIII.

Age.	Height.	Girt.	Contents.			Distance	Number of trees on an acre.	Contents of the whole.	Number to be cut out	Contents.
Years	feet.	inch.	feet.	in.	pts.	feet.		feet.		feet.
12	18	3	1	1	6	4·5	2151	2419	941	1058
16	24	4	2	8	0	6·	1210	3226	436	1162
20	30	5	5	2	6	7·5	774	4031	237	1234
24	36	6	9	0	0	9·	537	4833	142	1278
28	42	7	14	3	6	10·5	395	5645	93	1329
32	48	8	21	4	0	12·	302	6442	63	1344
36	54	9	30	4	0	13·5	239	7249	46	1395
40	60	10	41	8	0	15·	193	8041	33	1375
44	66	11	55	5	6	16·5	160	8873	26	1441
48	72	12	72	0	0	18·	134	9648	20	1441
52	78	13	91	6	6	19·5	114	10435	16	1464
56	84	14	114	4	0	21·	98	11204	12	1372
60	90	15	140	7	6	22·5	86	12093	11	1546
64	96	16	170	8	0	24·	75	12800		

Remarks. It is difficult in thinning plantations, to leave the trees at nearly equal distances. The distances stated in all these tables must be considered the average distances. If, for instance, there be 302 trees on an acre, their average distance will be 12 feet, although few of them may stand at exactly that distance.

Table 9 for thinning woods. If the trees be first planted 4 feet apart, and be periodically thinned out to the distance of one fourth of their height until they are 28 feet high, and to one third of their height afterward, and they increase 12 inches in height and 1 in circumference annually, the number of trees on an acre, and the number to be cut out at the ages and heights undermentioned, and the number of feet they will then respectively contain in the bole, when measured to the top of the leading shoot, will be as under, viz.

TABLE

TABLE IX.

Years old & feet high.	Girt.	Contents.			Distance.	Number of trees on an acre.	Contents of the whole.	Number to be cut out.	Contents.
	inches.	feet.	in.	pts.	feet.		feet.		feet.
16	2	0	5	4	4	2722	1209	980	435
20	2¼	0	10	5	5	1742	1512	532	461
24	3	1	6	0	6	1210	1815	322	483
28	3½	2	4	7	7	888	2115	453	1078
30	3¾	2	11	1	10	435	1271	133	388
36	4½	5	0	9	12	302	1528	80	404
42	5¼	8	0	5	14	222	1783	52	417
48	6	12	0	0	16	170	2040	36	432
54	6¾	17	1	0	18	134	2289	26	444
60	7½	23	5	2	20	108	2530	18	421
66	8¼	31	2	4	22	90	2807		

Observations on Table IX.

Remarks. On examining several oak woods, it appeared to me, that the distance of one third of their height was not too much, where the trees were from 30 to 40 feet high and upwards. I have therefore calculated a table according to the distance of one fourth of their height, till they are 28 feet high, and according to the distance of one third of their height afterward.

The timber to be thinned out before the age of 28 years will be the same as in Table VI; but at 28 years of age there are 583 feet more to be cut out according to this table than at the same age in Table VI; there will however be less to be cut out between the ages of 28 and 60 years of age. But if the trees in this table, in consequence of having more room, were to increase 1¼ inch in circumference annually, instead of 1 inch after they are 28 years of age, the produce of an acre at 60 years of age would equal the produce stated in Table VI at the same age; taking into consideration, that the value of the 583 feet excess cut out at 28 years of age would then be more than quadrupled, if the money were placed out at 5 per cent compound interest. A considerable

siderable additional increase in circumference may certainly be expected, in consequence of the trees having almost double the room in which to extend their branches, and for the admission of those powerful agents, sunshine and air.

(To be continued.)

IX.

Observations on Saturn's Ring: by Mr. LAPLACE*.

Two conditions necessary to the permanency of Saturn's ring. 1st.

TWO conditions are necessary, to maintain the ring of Saturn in equilibrio round that planet. One respects the equilibrium of its parts: which requires, that the particles on the surface of the ring should not have a tendency to separate from it; and that, supposing this surface to be fluid, it should preserve itself by the different forces with which it is actuated. Without this the continual effort of its particles would ultimately detach them, and the ring would be destroyed, like all those works of nature, which have not in themselves a cause of stability able to resist the action of the forces that operate against it. In the third book of my *Mécanique céleste* I have proved, that this condition can be fulfilled only by a rapid rotary motion of the ring in its plane, and round its centre, still a little distant from that of Saturn. I have likewise shown, that the section of the ring by a plane perpendicular to its own, and passing through its centre, is an ellipsis elongated toward this point.

2d condition.

Indifferent equilibrium of a hollow sphere.

The second condition regards the suspension of the ring round Saturn. A hollow sphere, and generally a hollow ellipsoid, the inner and outer services of which were similar and concentric, would be in equilibrio round Saturn, whatever point of the concavity were occupied by the centre of the planet. But this equilibrium would be *indifferent*; that is, if disturbed, it would have no tendency either to

* Journal de Physique, vol. LXIX, p. 241.

resume

resume its original state, or to depart from it: consequently the slightest cause, such as the action of a satellite, or of a comet, would be sufficient to precipitate the ellipsoid on the planet.

Does not hold with regard to a ring.

The indifferent equilibrium, which would take place for a hollow sphere enveloping Saturn, does not exist for a circular zone surrounding the planet. I have shown, in the book above quoted, that, if the two centres of a circular ring and the planet did not coincide, they would repel each other, and the ring would ultimately fall upon Saturn. The same thing would take place, whatever the nature of the ring might be, if it were without a rotary motion. But if we conceive, that it is not similar in all its parts, so that its centre of gravity does not coincide with the centre of its figure; and if we farther suppose, that it has a rapid rotary motion in its own plane; its centre of gravity itself will turn round the centre of Saturn, and gravitate toward this point as a satellite, with this difference, that it can move in the interior of the planet. Thus it will possess a stable motion.

The ring therefore has a rapid rotary motion.

Accordingly the two conditions I have mentioned concur to show, that the ring turns in its plane, on its own axis, and with rapidity. The time of its rotation must be nearly that of the revolution of a satellite moving round Saturn at the same distance with the ring; and this time is about 10 sexagesimal hours and a half. Mr. Herschel has confirmed this result by his observations. But how can we reconcile these observations, and this theory, with the observations of Mr. Schroeter, in which certain points of the ring, more luminous than the rest, have appeared a long time stationary? I conceive it may be done in the following manner.

This apparently inconsistent with the stationary spots observed by Schroeter.

Attempt to account for these.

The ring of Saturn is composed of several concentric rings. Powerful telescopes show two very distinctly, which are confounded together by iradiation in weak telescopes. It is very probable, that each of these rings is itself formed of several, so that the whole may be considered as an assemblage of various concentric rings. Such would be the aggregate of the orbits of the satellites of Jupiter, if each left behind a permanent light in its path. The separate rings, like these orbits, must be variously inclined to the equator of the planet: and then their inclinations and the position

of their nodes would change in longer or shorter periods, that would embrace several years. Their centres must equally oscillate round that of Saturn, and these circumstances together must at length alter the apparent figure of the rings as a whole. Their rotary motion does not perceptibly change this figure, since it only replaces one luminous part by another in the same plane. It is very probable that the phenomena observed by Mr. Schroeter are owing to variations of this kind. But if a point more or less luminous than the rest adhere to the surface of one of the separate rings, this point must move as rapidly as the ring, and appear to change its position in a few hours. We may presume with much probability, that it was a point of this nature, which Herschel observed.

Appearances of Saturn's ring deserve farther examination.

I would invite those observers, who have powerful telescopes, to examine the appearance of Saturn's ring with this view. The variety of these appearances greatly puzzled geometricians and astronomers, till Huygbens found out their cause. The ring at first exhibited itself to Galileo under the form of two small appendages adhering to the body of Saturn: and Descartes, who had an unfortunate propensity for explaining every thing in his Principles of Philosophy, ascribes, in the third part of that work, the stationary state of these supposed satellites to Saturn's always presenting the same face to the centre of his vortices. We now know, that this state is repugnant to the law of universal gravitation; and this reason would be sufficient, to induce us to reject the explanation of Descartes, even if we did not know the cause of these appearances. I do not believe, that the ring is immovable, though this would be less inconsistent with that grand law of nature; and I have no doubt that farther observations; made with the view I have just mentioned, will confine the results of the theory, and the observations of Herschel.

X.

X.

On the Mines of Sardinia: by the Count De Vargas, *President of the Italian Academy, &c.*[*]

Primitive mountains of Sardinia.

THE district of Barbagia and the province of Ogliastro are composed of granitic mountains, which extend in the form of amphitheatres from the seashore to the summit of Corruboi. These districts display to the mineralogist a vast and instructive study of primitive mountains.

Secondary mountains.

Other chains are of secondary formation, and their ramifications traverse the island in different directions. At every step they exhibit phenomena, which cannot but throw much light on geology.

Volcanic productions.

Lastly, many volcanic productions are found in the vicinity of Guisos, Santa Catharina de Pitturni in the territory of Cuglieri, and San Lussurgiu.

Ores.

But what particularly deserves attention is the great number of metallic veins, which are seen every where in great profusion. All the historians of Sardinia have spoken of this abundance of metallic ores. They were known from the remotest antiquity, for the remains of the labours of the Carthaginians and Romans in working them are still to be seen.

Gold mines.

Formerly, no doubt, gold mines were worked in the island, since one of the interior provinces still bears the name of the Gold Country; but none are now known there.

Silver mines.

Silver mines abound more or less in almost all the provinces. The mountain of Argentiera de Nurra exhibits another very distinct vein, nearly a mile long. This vein is of gray silver ore: its gangue is barytes. The vicinity of the sea, and abundance of wood in this part, are deserving attention. Horn silver is found in several places, as at Sarabus. Native silver too occurs, as near the bridge of San Nicolas, mixed with vitreous or sulphuretted silver.

Silver mixed with lead.

All the lead mines too contain more or less silver. There are some near the river Maggiore at Sarabus, which yield as much as eight or nine ounces in the hundred weight.

[*] Journal de Physique, vol. LXVII, p. 367.

Others contain but one, two, or three ounces. But the most celebrated of all is in the district of Tulana, which is said to yield seventy per cent of pure silver. This belongs to several private persons, who work it in secret.

Copper mines. Copper mines are tolerably abundant in Sardinia. The copper is generally in the form of pyrites. In the district of Sinla are very beautiful malachites.

Iron. Sardinia contains a large quantity of mines of excellent iron: but the most considerable is that of Arsana, which contains a magnetic iron of superior quality. There is another mine of magnetic iron in a mountain of porphyry at Trulada.

Lead. Lead mines abound in Sardinia, and all contain some portion of silver. The most considerable is that of Monteponi near Iglesias. It yields 60 or 64 per cent of pure metal. The lead mines of Sarabus are not less interesting.

Zinc. Blende or sulphuret of zinc, occurs mixed with galena.

Native quicksilver. On repairing the buildings of a convent at Oristono, native mercury was found in a bed of clay. Some persons say too, that native mercury was found on repairing the public prisons. Chaptal speaks of some having been found in a bed of clay on digging for the foundation of some buildings at Montpellier.

Antimony. There are a number of mines of antimony at Balland, and at Escala Plana.

Manganese. A mine of manganese has been discovered at San Pietro.

Coal. Coal has been found at Tanara near Forni, and at Corruboi.

XI.

Analysis of various Minerals, by Mr. KLAPROTH*.

Black crystallized augite of Frascati.

Pyroxene described. ONE of the principal varieties of augite is that found in fine black crystals in fissures in the Latian mountains, near Rome, particularly near Frascati, and formerly called black volcanic schoerl.

Its figure is commonly a hexaedral prism, bevelled at the extremities, the two faces of the bevel answering to the two

* Abridged from the Annales de Chimie, vol. LXVII, p. 225, &c. Trans. from Gehlen's Journal.

lateral

lateral edges of the prism †. Mr. Haüy has described this species under the name of *pyroxene*, the chief varieties of which are the *bisunitaire* and the *triunitaire*.

The surface of these crystals is smooth, sometimes shining, at other times only partly so. Interiorly they have a very glassy lustre.

They are hard, easily broken, and their fracture perfectly conchoidal. When rubbed to powder their colour is a greenish gray.

Their specific gravity is 3·4.

Before the blowpipe, on charcoal kept at a red heat, the angles and edges ultimately become rounded. Treated with the blowpipe.

A *a*. A hundred grains, reduced to an impalpable powder, were heated red hot with twice their weight of caustic potash. The matter did not enter into fusion. It was of a brown colour, and gave a slight green tinge to the water, with which it was diluted. On supersaturating the liquor with muriatic acid, a complete solution was obtained. On evaporating to dryness, and redissolving in water, the silex was separated. After being heated red hot, it weighed 48 grains. Analysis. Silex.

b. The solution was precipitated by ammonia; and the brown precipitate, while still wet, was boiled in a caustic lixivium. The alkaline liquor, mixed with muriate of ammonia, let fall alumine, the weight of which, when purified, was 5 grains. Alumine.

c. The brown residiuum was dissolved in nitric acid, the solution diluted with a great deal of water, and carbonate of soda added. The oxide of iron precipitated, and heated red hot, weighed 12 grains. Oxide of iron.

d. The supernatant liquor was decomposed at a boiling heat by carbonate of soda. The precipitate obtained, and heated red hot, weighed 10¼ grains. It had assumed a reddish colour. Being dissolved in nitric acid, it left behind oxide of manganese, weighing one grain after calcination. Oxide of manganese.

e. As the nitric solution appeared to contain magnesia and lime, oxalate of potash was poured in, till no farther precipitate ensued. The oxalate of lime, collected and heated red Lime.

† The places of these two edges are occupied by trapezoidal facets in the triunitary variety, the prism of which has eight sides instead of six.

red hot, yielded 4½ grains of lime. The remaining liquor, being decomposed by carbonate of soda, yielded 5 grains of calcined magnesia.

Magnesia.

f. The muriatic solution, decomposed by ammonia in experiment *b* was precipitated boiling by carbonate of soda. The precipitate washed and dried weighed 44¾ grains. This was neutralized by sulphuric acid, and evaporated to dryness. The hardened mass was triturated and lixiviated gradually with a little water. The solution being evaporated, left sulphate of magnesia, which was decomposed by carbonate of soda, and 9 grains of carbonate of magnesia were obtained. These, being deducted from the 44¾ of the first precipitate, left for the carbonate of lime 35¾ grains, which amount to 19¼ of lime. The 9 grains of carbonate of magnesia being heated red hot, 3¼ grains of magnesia were obtained.

More lime and magnesia.

B. Eighty grains of this stone reduced to an impalpable powder were heated red hot with an ounce of nitrate of barytes. The calcined matter was triturated with boiling water, dissolved in muriatic acid, and then precipitated by carbonate of ammonia. The filtered liquor was evaporated to dryness, and the salt volatilized in a platina crucible. An earthy salt remained, which was redissolved in water, and decomposed by carbonate of ammonia. The filtered liquor was evaporated anew, and the salt volatilized by heat. A slight trace of muriatic neutral salt remained, which was found to be muriate of potash, by its forming a few grains of a triple salt with a solution of platina.

Potash.

A hundred parts of augite of Frascati therefore contain

Component parts.

Silex	A *a*		48
Lime	*e*	4·5	24
	f	19·5	
Magnesia	*e*	5	8·75
	f	3·75	
Alumine	*b*		5
Oxide of iron	*c*		12
Oxide of Manganese	*d*		1
Potash	B	*a* trace	
			98·75

This

This analysis nearly agrees, both in the nature of the component parts, and in their proportions, with that which Mr. Vauquelin has given of the crystallized black augite of Etna; in which he found

Pyroxene from Etna analysed by Vauquelin.

Silex	52
Lime	13·20
Alumine	3·33
Magnesia	10
Oxide of iron	14.66
——— manganese	2
	95·19

Hence we may consider these two stones as one variety, though the quantities of lime and alumine are less in that of Etna, and those of the other component parts on the contrary greater.

Analysis of Melanite.

Another stone is met with at Frascati, and at Albano, near Rome, which is found in single detached crystals, and has been called black garnet.

Black garnet.

Its form is that of the emarginated garnet of Haüy. By trituration it yields a brownish gray or blackish powder. Its specific gravity is 3·7. When heated red hot in a crucible it undergoes no observable change; but before the blowpipe it rounds gradually into a globule.

Described.

Action of heat on it.

It is unnecessary to give the analysis in detail, as it was conducted like the preceding, except that the oxide of iron was precipitated by succinate of ammonia, and the following method was pursued for detecting a fixed alkali.

Analysis.

Sixty grains were decomposed by muriatic acid; but this was not effected completely till after several repeated digestions. After separating the silex, the solution was decomposed by carbonate of ammonia. The ammoniacal liquor was evaporated to dryness, the residuum redissolved in water mixed with carbonate of ammonia, filtered, and evaporated again. The salt, volatilized in a platina crucible, did not give the least trace of a fixed alkaline salt.

Examination for alkali.

The

The component parts of the melanite were found to be

Component parts.

Silex	35·5
Lime	32·5
Alumine	6
Oxide of iron	24·25
——— manganese	0·4
	98·65

This analysis agrees very closely with that of Vauquelin. The melanite differs greatly therefore from augite, and particularly in containing no magnesia.

Bohemian garnet.

In a note Mr. Klaproth observes, that his analysis of the Bohemian garnet, now called *pirop*, has been given in several French works as his analysis of the melanite; and he has found, since the publication of the 2d volume of his Essays, that about 2 per cent of chromic acid should be added to the component parts of the Bohemian garnet as there given.

Analysis of the staurolite (staurotide of Haüy).

Cruciform schoerl.

Mr. Klaproth has analysed two varieties, one red the other black, both from St. Gothard.

Red.

The proximity of the red staurolite to the cyanite is very remarkable. These two substances are frequently crystallized together, so as apparently to form but one stone. When this is the case, the staurolite becomes a little translucid at the end of the prism.

Brown.

The brown staurolite of Quimper, in the department of Morbihan, as well as that of Finistere, in France, serves as a connecting link between the black and red varieties. In this country conjoined crystals are much more common than single ones; and they commonly cross each other at right or at oblique angles (the *staurotide rectangulaire* and *obliquangle* of Haüy). Frequently the crystals joined together are of the same size; but often one is smaller than the other, and seems implanted in the larger.

Black.

The specific gravity of the black staurolite was 3·51. It experienced

experienced no change of colour, weight, or figure, by calcination. Its component parts were

Silex	37·5	Component parts of the black,
Alumine	41	
Oxide of iron	18·25	
Magnesia	0·5	
Oxide of Manganese	0·5	
	97·75	

The specific gravity of the red staurolite was 3·765. Its component parts

Silex	27	of the red.
Alumine	52·25	
Oxide of iron	18·5	
——— manganese	0·25	
	98	

Analysis of hypersten, called Labrador hornblende.

Mr. Haüy was the first who distinguished this stone from hornblende. He had classed it with the metalloid diallage, or our bronzite; but he has lately shown, that it differs both from hornblende, and from the diallage, or smaragdite. He designates it under the name of metalloid reddish brown lamellar hypersten. Labrador hornblende.

Its specific gravity I find to be 3·39. Before the blow-pipe it is infusible, but its semimetallic lustre is turned blackish. If exposed to a red heat after trituration, the powder, which was of a deep ashen gray, acquires a brown red colour, and loses one per cent of its weight. Spec grav. Action of heat.

The results of its analysis were Component parts.

Silex	54·25
Magnesia	14
Alumine	2·25
Lime	1·5
Oxide of iron	24·5
Water	1
Oxide of manganese, a trace	
	97·5

Analysis

Analysis of the stangenstein of Altenburg (pycnite of Haüy).

White schoerl of Altenburg.

As this stone has been termed shoerllike beryl, Mr. Klaproth examined it for glucine, after this earth had been discovered in the beryl and emerald by Vauquelin; but he could not find the least trace of it. He had formerly observed the great difference between it and beryl, when they were exposed to the heat of a porcelain oven; as this stone lost twenty-five per cent, and the beryl but one. This led him to conclude, in 1800, that it contained the same volatile matter as the topaz. Mr. Bucholz, apparently without knowing this had been mentioned by Mr. Klaproth, found it to be the fact; which was afterward confirmed by Vauquelin. Mr. Klaproth, having since analysed it with great care, obtained the following results.

Component parts.

Silex	43
Alumine	49·5
Oxide of iron	1
Fluoric acid	4
Water	1
Loss	1·5
	100

Allied to the topaz.

Vauquelin.

This analysis shows, that it is nearly allied to the topaz. The 3·3 per cent of lime found by Mr. Vauquelin are supposed by Mr. Vauquelin himself, to have been owing to the impurity of his specimen.

Analysis of the reddish tourmalin of Moravia.

Reddish Moravian tourmalin.

This tourmalin is found in the mountain Hradisko, near Roczna, imbedded in a compact whitish gray quartz, or in lepidolite. It is in the form of prisms, or needles, of a peach-blossom colour, which verges in several parts to greenish, yellow, and gray white. As it is met with immediately under lepidolite, it has been taken for lepidolite crystallized; and it is under this name that Estner has given a very minute description of it, to which I refer the reader. By some preliminary trials it was soon found, that this

this stone was not a lepidolite; and it was then classed with the schoerlaceous beryl, or stangenstein. Mr. Haüy placed it with the red schoerl of Siberia, or siberite, with much more reason, as will appear by the following analysis: and he even classed it among the tourmalins, because its crystals have the property of attracting and repelling light substances, when they are heated. As it is not fusible like the tourmalin however, he distinguished it by the epithet apyrous.

The specific gravity of the crystals detached from the quartz varies, according as they are more or less old, from 2·96 to 3·02. Spec. grav.

Its component parts are

Silex	43·5	Component parts.
Alumine	42·25	
Oxide of Manganese	1·5	
Lime	0·1	
Soda	9	
Water	1·25	
	97·6	
Loss	2·4	
	100	

The component parts of this stone therefore, and their proportions, completely justify Mr. Haüy, even in a chemical view, for classing it with the siberite, or apyrous tourmalin; since, from a recent analysis of the latter by Mr. Vauquelin, it is composed of Rubellite tourmaline.

Silex	42	Component parts.
Alumine	40	
Oxide of manganese, a little ferruginous,	7	
Soda	10	
Loss	1	
	100.	

(To be continued.)

XIII.

XII.

Method of curing the Footrot in sheep. By Mr. RICHARD PARKINSON, *of Walworth**.

SIR,

THE enclosed is the recipe for the cure of the footrot in sheep, certified by the person who was my shepherd at the time I put the method into practice.

I am, Sir, your very obedient servant,

18, *Harford Place, Walworth,* R. PARKINSON.
April 6, 1807.

To Cure the Foot Rot in Sheep in the best and most effectual Manner.

Cure for the footrot in sheep.

In sheep thus affected, pare their hoofs, leaving no hollow to hold dirt; if there be matter formed, be particularly careful to let it out; after which, take some stale urine and wash their feet clean from dirt, and wipe them with a sponge; then put the sheep into a house or shed, the floor of which has been previously spread about two inches thick with quick lime, reduced to powder by a small quantity of water. The fresher the lime is from the kiln the better. Let the sheep stand upon it for six or seven hours, and the cure will be effected.

Testimony of its efficacy.

A certificate, dated March 27, 1807, from Joseph Dunnington, stated his being shepherd to Mr. Parkinson, at Slane in Ireland, in the year 1803, and that he then witnessed the efficacy of the above remedy on a large flock of sheep.

Farther certificates from the Earl of Conyngham, from Mr. Stephen Parkinson, and from Joseph Preston, shepherd to Mr. John Parkinson of Bolingbroke, confirmed the above statement.

XIII.

On the Use of the Italian Poplar for supporting the Vine and Hop†.

Poplar used to support the vine.

IT is well known, that in Italy the poplar is employed as a support to the vine. When thus used, it is frequently

* Trans. of the Soc. of Arts, vol. XXVI, p. 126. The silver medal of the Society was voted to Mr. Parkinson.

† Sonnini's Bibliotheque Physico-écon. Nov. 1808, p. 311.

lopped,

lopped, that its branches may not spread so as to be injurious.

and the hop. Mr. Hubert, counsellor of the bailiwick of Iphofen, in Franconia, says, in a paper on the cultivation and use of the poplar, that it may be planted to support the hop, and would be an advantageous substitute for the poles usually employed, which occasion a considerable consumption of wood.

Carolina poplar recommended. The poplar, particularly the Carolina, *populus angulata*, grows in the poorest soil, its leaves are good food for cattle, and its wood is employed for various purposes. Much would be saved therefore by employing it in our hop-grounds. We may presume it would not deprive the hop of its nutriment; and its leaves, after having sheltered the hop from injurious winds, would serve as manure when they fell.

but perhaps the Italian preferable. Every species of poplar does not appear to be equally well adapted to the support of the hop. The Italian poplar, *populus fastigiata*, perhaps deserves a preference. Its advantages. Beside its growth being very rapid, as it attains the height of 60 or 70 feet in 20 years, its branches do not spread so much as those of other species. If barked a twelvemonth before it is felled, or indeed when cut down if it be at the time the sap is rising, its wood acquires great hardness, and Wood. it is not liable to be injured by the worm. As fuel indeed its quality is but indifferent, as it does not afford much heat.

XIV.

Analysis of the Root of Valerian: by Mr. TROMMSDORFF*.

Valerian root loses 0·75 by drying. Essential oil. THE root of valerian, *valeriana officinalis L.*, loses 0·75 by drying. Twelve pounds of the dried, or 48 of the fresh root, distilled with water, yield 2 ounces of volatile oil. This oil is very fluid, and of a greenish cast. Its smell is strong, penetrating, and more camphory than that of the root. Its spec. grav., at 20° R. [77° F.], is 0·934. Its taste is aromatic, and camphory, but not burning. The action of light

* Annales de Chimie, vol. LXX, p. 95.

turns

turns it yellow. Nitric acid does not inflame it, but converts it into a very odoriferous resin, of an orange yellow, and a bitter yellow substance. If a larger quantity of nitric acid be employed, crystallized oxalic acid is obtained.

Expressed juice. The expressed juice of the fresh root is turbid, very odoriferous, and lets fall a little fecula. Caloric separates from it a little albumen. The filtered juice contains neither gallic acid, nor tannin, nor common extractive matter, but a

Peculiar Principle. peculiar principle, soluble in water. This principle is insoluble in ether, and in rectified spirit: it forms precipitates with the soluble salts of lead, silver, mercury, and antimony; but it does not precipitate sulphate of iron, or solution of alum.

Method of obtaining this separate. To obtain this principle separate, the filtered juice is to be precipitated by acetate of lead. The precipitate being first well washed, is to be diffused in distilled water, and sulphuretted hidrogen gas passed through it, till the whole of the metal is separated. The liquor is then to be filtered, and the hidrogen gas volatilized by ebullition. The solution is then to be evaporated to dryness, on a water bath.

Gummy extract. The expressed juice contains likewise a quantity of gummy extract.

Resin. When the roots, after expression of the juice, have been exhausted by boiling water, the residuum, treated with highly rectified alcohol, yields a black resin, that has the smell of leather, and an acrid taste. This resin is very fusible, and readily takes fire. It dissolves in alcohol and ether, and likewise in oil, both volatile and fixed. The dried root contains about a sixteenth of it.

According to Mr. Trommsdorff's analysis, a pound of the dried root contains

Fecula	144 grains.
Peculiar extractive matter	1152
Gummy extract	864
Black resin	576
Volatile oil	96
Woody substance	6384
	9216

SCIENTIFIC

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT the meeting of this Society, on Saturday, the 21st of July last, Mr. Campbell of Carbrook read some observations on the cause of the antilunar or inferior tide, impugning the Newtonian theory on that subject; and Dr. Thomas Thomson read a paper on natural carburetted hidrogen gasses, showing, that they contain different quantities of carbon, but no oxigen. Antilunar tides. Carburetted hidrogen contains no oxigen.

Preface to the Encyclopedia Britannica.

Dr. Kirby has sent me a printed copy of a letter from himself to Dr. Millar, editor of the 4th edition of the Encyclopedia Britannica, which has been inserted in most of the Edinburgh newspapers. Dr. Kirby complains, that injustice both of omission and assertion has been done him as one of the composers of that work, one tenth part at least having been either written or revised by him. Dr. Millar replies, by admitting the errours of the preface, which he condemns in the strongest terms, and states, that he was not permitted to draw it up, because he refused to have his preface revised and corrected by persons he considered as very incompetent. I am sorry, that for obvious reasons I am prevented from giving more than this abridged statement of a business, in which the interests of science appear to be materially concerned. Preface to the Encyclopedia Britannica.

St. George's Hospital, and George Street, Hanover Square.

The latter end of the first week of October, the usual courses of lectures on the practice of physic, therapeutics, and chemistry, will recommence in George Street; *viz.* the medical lectures at 8, and the chemical at 9 in the morning: by George Pearson, M.D., F.R.S., senior physician of St. George's Hospital; of the College of Physicians, &c. Medical lectures.

Note. Clinical lectures are given on cases of patients in St. George's Hospital, as usual.

METEOROLOGICAL JOURNAL,

For SEPTEMBER, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

AUG. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
27	65°	62°	70°	54°	30·09	Fair	Fair
28	60·5	65	68	54	30·16	Ditto	Ditto
29	62	67	68·5	60	30·15	Ditto	Cloudy
30	65·5	67	72	60	30·03	Ditto	Ditto*
31	65·5	71	75	63	29·92	Ditto	Ditto†
SEPT.							
1	68	72	77	62·5	29·88	Ditto	Fair‡
2	70·5	73	79·5	63·5	29·94	Ditto	Ditto
3	67	62	62·5	55	29·85	Rain	Rain
4	61	61·5	62	50	29·70	Fair	Fair
5	55	60·5	62	53·5	30·02	Ditto	Ditto
6	60	56	66	48·5	30·00	Ditto	Ditto
7	55·5	58·5	64·5	48	30·35	Ditto	Ditto
8	54·5	60	64	48	30·18	Ditto	Ditto
9	53·5	61·5	68	52·5	30·10	Ditto	Ditto
10	57	55	66	48	29·96	Rain	Ditto
11	53·5	54·5	65	48	29·90	Cloudy	Rain
12	53	52·5	57	44·5	29·69	Rain	Fair
13	50	57	59	54	30·07	Ditto	Ditto
14	60·5	61	66	48	30·16	Fair	Ditto
15	53	56·5	59·5	49·5	30·37	Ditto	Cloudy
16	57	59	62	56	30·27	Rain	Ditto
17	61	62	65	53·5	30·11	Fair	Ditto
18	58	61	63	58	30·05	Ditto	Ditto
19	60	62	64	54	30·10	Ditto	Ditto
20	58	60	63·5	56	30·12	Ditto	Ditto
21	61·5	62	65	55	30·09	Ditto	Ditto
22	61	61	67	51·5	30·03	Ditto	Ditto§
23	56·5	57	59	51	29·92	Rain	Fair
24	56	59	61·5	55	30·10	Fair	Cloudy
25	59·5	60	65	53·5	30·15	Ditto	Fair
26	60·5	60	66	52·5	30·11	Ditto	Ditto

* Rain at 11. Thunder and lightning in the Night.
† Thunder, lightning, and heavy rain.
‡ Cloudy in S W. § Lightning.
Rain, 1·89 of an inch, since last Journal.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1810.

ARTICLE I.

On the Electric Column. By J. A. De Luc, *Esq. F.R.S.*

Part II.

On the Electric Column, as Aerial Electroscope.

I Have said in the preceding paper, that, the *sticking* of the *gold leaves* to the side of the electroscope being an obstacle to regular observations of their *strikings*, which however I considered as the most proper manner of observing the variations of this instrument, I had intended to increase its power, so far as to produce the motion of a small metallic *ball*, in hopes that the latter would not *stick*.

The sticking of the gold leaves disadvantageous.

I began this attempt by uniting together in one the three *columns* of 200 groups mentioned in the *first Part*, which I had before used by only connecting them with one another; and I thus formed the *column* of 600 groups making a part of the instrument represented by the *figure* annexed to the first part: see Pl. III. With this *column* I made the first trial of procuring the motion of such a small *pendulum* in the following manner. I connected with each extremity of the *column* a wire terminated by a small brass *ball*; and

Attempt to move a metallic ball.

each wire being movable, I could bring the two *balls* in front of the instrument, within the distance of each other that might be found convenient; and I suspended, by a silk thread, a small gold *bead*, which I could easily bring between the *balls*, either at the middle distance, or nearer one than the other.

Unsuccessful trials with an insulated ball.

The apparatus being thus prepared, I tried for a long time to make it answer my purpose, but without success. When the two *balls* were near each other, the *bead* struck them alternately with such a rapidity, that it was impossible to count the number of oscillations in a determined time; a necessary condition for my purpose. I increased the distance of the *balls*, but I was disappointed in a different manner: when the *bead* was equally attracted on both sides, it sometimes remained motionless for hours in the middle of the interval; and when the attraction became stronger on one side, the *bead*, drawn that way, struck one of the *balls*, some oscillations then began, still too rapid and very irregular, but at the end of a little time, they ceased again, the bead remaining motionless at the middle point. I tried various distances of the *balls*, and also different degrees of approximation of the *bead* to either of them: sometimes there was an appearance of success, but at last the bead was again at rest in the middle space, or it *stuck* to one of the *balls*. This want of success persuaded me, that a *neutral* pendulum, such as was the *bead* suspended by a *silk* thread, could never answer the purpose of the regular *strikings*, which were necessary; that the *bead* was to be connected, by a *metallic* wire, with one of the extremities of the *column*, near a *ball* united with the latter, and to *strike* against another *ball* connected, either with the other extremity of the *column*, or with the ground; the latter of which modes I first adopted.

Mr. Forster had succeeded with a longer column, to produce a continual chime.

But before I proceed, I must mention, that while I was employed in these trials at Windsor, Mr. B. M. Forster had succeeded at Walthamstow, near London, with a power of 1500 groups of the same diameter as mine, divided into three chaplets, as described in my first paper presented to the Royal Society, to set in motion a brass ball, suspended by a silk thread between two small insulated bells, connected with

with the two extremities of this long *column*. Mr. Forster has described this new kind of *chime* in a letter to Mr. Tilloch, published in the *Phil. Magaz.* of the latter, mentioning this singular circumstance, that the ringing began on the jubilee day. Since that time I have had the pleasure of being personally acquainted with Mr. Forster, and he has lately informed me, that, having mounted again this chime on the 25th of March, it has not ceased to ring ever since. This is a curious application of the property of the *column*, but, as I have explained, it cannot answer the purpose of regular observations.

For the execution of the above mentioned plan, I first made the following addition to my apparatus. At the top of one of the pillars of the *column*, on its side A (the positive extremity, I fixed a brass piece 13, held there by a ferrule, and projecting forwards about $1\frac{1}{2}$ inch: on this projection is fixed, by a screw, another brass piece, having on one side a vertical groove 14, in which is held, by a pin, a brass rod, at the lower part of which is a large brass *ball* 15, which can be moved backwards and forwards, in order to bring it to the convenient point, where it remains steady, by the friction of the top of the rod in the groove. From this top projects a brass loop 16, to which is suspended a gold *bead* 17, by the thinnest silver wire, such as is used for cross wires in telescopes; and by moving properly the *ball* 15, the *bead* is made to hang close to it; without leaning against it: this is done while both the *bead* and the *ball* are *neutral*, by handling the latter for this adjustment. Addition to the apparatus.

My purpose having only been at first, that the *ball* 18 should be in communication with the *ground*, I produced this communication in a simpler manner than is represented in the *figure*; having changed it since for a purpose that I shall explain; but the difference is here of no consequence: it was then only held at the top of a brass stem, fixed to the end of a thick slip of lead 19, 19; movable backward and forward between pins 20, 20, 20, in order that the distance of this *ball* from the *ball* 15 might be changed, as should be found proper for the *strikings* of the *bead*.

This apparatus was finished in the beginning of last

spring; but it was too late for the most important observations; especially as the apparatus itself was far from being settled. Before that season, the effect of the *column* had been so great, that the *gold leaves* of the electroscopes, either on one side or the other, *struck* sometimes in the afternoon 60 times in a minute, even in glass tubes of $1\frac{1}{2}$ inch diameter; but now they seldom *struck* more than once in a minute, *sticking* always as usual (which was the reason why I had given up the *gold leaves*): however the effect was still sufficient to try the new apparatus.

The ball would still stick.

This again gave me much trouble: for though at first it appeared to answer my purpose; as the gold *bead*, receding from the *ball* 15, struck the ball 18, fell and returned again, with the usual changes in the *frequency* which were to be the object of observation; the *bead* at last *stuck* to the *ball* 18. I tried whether, by increasing the distance of the latter, the *bead*, thus drawn farther out of the vertical line, would have more power to resist the cause of its *sticking*; which it is difficult to understand, as it is not the case when set in the same motion by a mechanical impulse. The *strikings* were less frequent; not so however as to prevent the observation, if the *sticking* had been prevented; but it again took place, and even more easily, as the *bead* arrived more slowly in contact with the *ball*. I did not succeed better by increasing the power (in a manner which I shall explain hereafter), though I could produce the *strikings* at a greater distance; so that, after much labour, I had some time despaired of success, when another idea occurred to me, which however did not succeed at first.

Attempt to produce the striking by the wire, not by the bead.

The general idea was, to produce the *strikings*, not by the *bead* itself, but by its silver *wire*, in a part at some height above the latter, by the *wire* meeting there the edge of a horizontal brass lamina in communication with the ground; in order that the *wire* being bent at that point, by the *bead* moving still farther, the latter should have a greater angular motion from that point, by a shorter *radius*, and have thus a greater tendency to fall back after being discharged; suppressing at the same time the *bal.* 18, to which it *stuck*: but by this suppression, the motion was so much diminished, that I was obliged to use again the *ball*; and then the

wire

wire itself *stuck* to the edge of the lamina. Following however farther the idea of discharging the *bead* by its *wire* meeting with the smallest *conducting* mass possible, I thought of substituting for the brass *lamina* a single silver *wire* like that of the *bead*; and at last I thus succeeded. This is the part of the *figure* which I am going to describe.

Farther addition to the apparatus.

The last alteration I found necessary to make in the apparatus, which is represented in the *figure*, not being made at that time, it must be supposed for the present, that the piece 28 is represented by the lead slip 19, 19. The piece 23 is a brass spring of about half an inch in breadth at the base 24, where passing under the bent part of an upright brass piece 25, it is thus fixed with the latter, by screws, on the base. The breadth of the spring 23 diminishes toward its end, where it is terminated by a brass wire *bow* 22; in this is stretched the thin silver *wire* 21, against which that of the *bead* comes to *strike*. The upright brass piece 25 has at the top a screw 26, pressing against the spring, and serving to produce small motions, backwards or forwards, of the horizontal wire 21, previously brought nearly to the proper distance, by moving the lead base 19, 19. The moment of the meeting of the two *silver wires* is to be an instant before the *bead* strikes the ball 18: then, by a jerk produced at the meeting of the *wires*, the *sticking* of the *bead* to the *ball* is effectually prevented.

The strikings affected by the least shake.

This was only finished in the beginning of last April; the *strikings* of the *bead* were then regular and uninterrupted, while there was no shake of the apparatus itself; but being on a table, I soon found, that by walking in the room, and also by the agitation of the air in opening and shutting the door, the motions of the *bead* were disturbed. This determined me to fix, against the side of the room which had a proper light, a glazed box, in which I placed the apparatus; and I fixed under it, at its level, a little table, in order to place there additional *columns*, which became necessary to increase the power of the instrument.

Journal of the performance of this instrument.

This apparatus being at last ready for regular observations, I began the meteorological journal which I had in view,

view, including with the *number* of the *strikings* of the *bead* in a certain time and the observation of the *barometer*, the degrees of the *thermometer* and of my *hygrometer* in the room, the only place of which it could be supposed that the changes of the *temperature* and of the degree of *moisture* might affect the state of the column: I shall copy here the journal of these observations during the few days in which the fundamental *column* of 600 groups still acted alone; after which time I was obliged to increase the number of the *groups*.

April			*Barom.*	*Thermom.*	*Hygrom.*	*Numb. of strik. in 10 min.*
8.	8	A.M.	29·7	57	41	11
	11		id.	60	id.	10
	1	P.M.	id.	60½	id.	12
	3		id.	63	id.	20
	5		id.	62½	id.	10
	7		id.	61	id.	8
	11		id.	61	id.	7
9.	8	A.M.	id.	58	42	11
	11		id.	60½	id.	9
	11	P.M.	id.	63	41	3
10.	11	A.M.	29·8	61	id.	6
	2	P.M.	id.	60	id.	5
	11		29·9	58	id.	2
11.	7	A.M.	id.	50	id.	3
	4	P.M.	30	56	id.	2
12.			30·1	No striking the whole day.		

Cessation of the striking.

This cessation of striking having lasted two days more, I judged, that we were entering into the season, when, in the two years before, the *strikings* of the *gold leaves* themselves had also ceased; so that in order to carry on the observations as long as possible towards the summer, it was necessary to increase the number of the *groups*. This I undertook without changing the situation of the fundamental instrument, which, on account of the necessary steadiness, and of being sheltered from currents of air, was to remain in the glazed box fixed against the side of the room.

room. I made use therefore of the little table above-mentioned, for placing on it additional columns, which I made upright like the common *pile*, as more easily managed; and knowing that with the same number of groups, the *strikings* would be accelerated by larger plates, but that it was a long and tedious operation to cut them round, I determined to make a *column* with square plates. For this purpose I bespoke some sheets of luminated *zinc* about the thickness of a card; but those which I received were so much puckered, that I despaired of their being fit for my purpose; however I obtained flat plates of them by a method which it may be useful to explain.

Sheets of zinc not smooth,

I procured a good pair of hand-shears, and with these I first cut the puckered sheets into slips of $1\frac{1}{4}$ inch breadth, as nearly as they could be traced upon such an uneven surface; and placing many of these slips upon one another between two pieces of hard wood, I pressed them with force in a vice, leaving them there for half an hour: they came out very flat, only not very straight, but this could be mended. *Zinc*, in this malleable state, having nearly the softness of *lead*, stretches laterally by that great pressure, and thus the puckerings are effaced. Making then straight, with a file, one side of the slips, I marked on this edge, with a divider, points at $1\frac{1}{4}$ inch distance; and by these points I traced with a square the plates to be cut with the shears: these pieces were distorted by the cutting; but placing them also over one another, by scores, between two thick plates of brass, and pressing them strongly in the vice, they became flat; and I had only to round a little the angles with a file, placing them again in the vice without the brass plate.

but may be made so by strong pressure.

In this manner, I made 300 *zinc* plates $1\frac{1}{4}$ inch square, and having cut an equal number of pieces of *Dutch-gilt paper* of the same size, I mounted this upright *column* between 4 glass rods covered with sealing-wax, fixed in a wooden base. This *column*, loose between the rods, is supported at the bottom on 4 insulating pillars $1\frac{1}{2}$ inch high, on which is first laid a brass plate with a projecting part of about 2 inches, at the extremity of which is a large hole for receiving the end of proper conductors; and for the same purpose,

An upright column added.

purpose, the column is terminated at the top by a similar plate, on which presses a screw in the common manner used for the *pile*. The top of this *column*, which is its *positive* extremity, was to be connected with the *negative* extremity of the horizontal *column*, and this required, that the box containing the latter should be opened in front: I therefore placed only a pane of glass on the side where the *bead* hangs, in order to guard it against the motions of the air.

The first use which I made of this additional *column* with *larger plates* was for the following experiments.

Exp. 16. *Exper.* 16. Placing the *negative* (or lower) extremity of the *column* composed of the large square plates, in communication with the ground; I connected its *positive* extremity with a *gold leaf* electroscope, and after having observed the *maximum*, soon produced, of its *divergence*, I substituted for this *column* 300 of the small groups of the horizontal *column*, by placing the communication with the ground at its middle point: this produced the same divergence as the former, but it required more time.

I could not compare directly the effects of the two *columns* with respect to the *frequency* of *strikings* of the *bead*, because at that time 300 groups of any size were no longer sufficient for producing them; but I compared the effects of the two *columns*, for this purpose, in the following manner.

Exp. 17. *Exper.* 17. I first repeated the observation of the *strikings* with the horizontal column of 600 groups, its *negative* extremity being in communication with the ground: there were 3 *strikings* in 5 minutes. I then took off the communication of this *column* with the ground, and connecting its middle point with the *positive* extremity of the column of 300 groups of the square plates, I placed the *negative* extremity of the latter in communication with the ground. This was again 600 groups, but 300 of them were of *larger* plates, and there were then 7 *strikings* in the same time; and thus was confirmed what I had judged of the effect of larger plates for increasing the *frequency* of the *strikings*.

Exp. 18. *Exper.* 18. I now connected the new *column* of 300 larger plates with the 600 groups of the horizontal column, leaving the communication with the ground at the *negative* extremity of the former. This was in a more favourable moment;

moment; for the addition of 300 of the small groups ought to have produced with the whole only 8½ *strikings* in 5 minutes, and there were 10.

From this increase of power, I expected a greater duration of observations in this season; but I soon saw a diminution in the frequency of the *strikings*, which disappointed me. I lost again much time in changing the arrangement of the apparatus, in order to take in also the two *columns* formed of *tinned-iron* plates, mentioned in my first paper: there were 700 of these groups, which produced in the *gold-leaves*, nearly the same *divergence* as the *column* of 300 zinc plates 1½ inch square: This being the only change that I could undertake for the season, I began another course of observations; and it will be seen in the following journal, how rapidly the effect went on diminishing.

Diminution of the strikings

went on rapidly.

May			Barom.	Thermom.	Hygrom.	Numb of strik. in 5 min.
10.	8	A.M.	30·15	63	40	14
	12		id.	67	id.	19
	2	P.M.	30·17	67	id.	17
	4		30·20	66	id.	16
	8		30·25	65	id.	12
	10		id.	65	id.	10
11.	7	A.M.	id.	59	41	7
	11		id.	64	39½	8
	2	P.M.	id.	67	id.	11
	4		id.	67	id.	9
	10		id.	64	39	4
12.	7	A.M.	30·19	57	39½	4
	11		30·18	60	39	4
	3	P.M.	30·15	65½	38¾	6
	10		30·10	65½	39	4½
13.	9	A.M.	30·05	63	40	7½
	1	P.M.	30·02	65	39½	7
	10		30	64	39¼	2
14.	9	A.M.	29·85	62	39½	5
	12		29·80	65	id.	7
	3	P.M.	id.	67	id.	8
	11		29.75	66	39	*no striking.*

May			Barom.	Thermom.	Hygrom.	Numb. of strik. in 5 min.
15.	7	A, M.	29·65	61	40	
		No *striking* the whole day.				
16.	7	A. M.	29·6	64	40½	½ or 1 in 10 min.
	11		29·65	63	39¼	5
		No *striking* in the evening.				
17.	7	A. M.	29·83	59	40	1
	10		id.	63	39¾	3½
	11	P. M.	29·65	66	id.	5
18.	7	A. M,	29·75	59	40	3¾
	12		29·80	63	39½	3½
	11	P. M.	29·95	61½	39	2
19.	7	A. M.	30·1	56	39½	*no striking.*
	4	P. M.	30·2	65	id.	4
		No *striking* in the evening.				
20.	8	A. M.	30·18	57	39¼	2½
	4	P. M.	30·08	67½	39	4
	10		30	67	38¾	2
21.	7	A. M.	29·9	60	40	½
	2	P. M.	29·83	66	39¾	5¼
	11		29·9	65	40	3
22.	7	A. M.	30·1	60	40½	2½
	5	P. M.	30·15	69	40	5½
	10		30·2	64	40½	4¼
23.	8	A. M.	30·3	60	40½	3¼
	12		id.	63	39½	4
		No *striking* for a long time.				
	7½	P. M.	id.	66	39	2
24.	6½	A. M.	30·38	59	39¼	1¾
	2	P. M.	30·32	66	39	4
	10½		30·3	65	id.	1½
25.	6½	A. M.	30·25	61	39¾	2½
	3½	P. M.	30·24	69	38½	3
	10½		30·28	68	39	2
26.	7	A. M.	30·23	59	40	2½

This

This great diminution in the *frequency* of the *strikings*, made me think of connecting the *ball* 18 with the *negative* extremity of all the *columns*, in order to see what increase it would make in the *frequency*: but luckily at the same time it occurred to me, that, by producing a speedy manner of changing this connexion of the *ball* for that with the *ground*, and inversely, it would be a mode of discovering variations in the *electric state* of the latter, by comparing in a short time its effect on the *strikings*, with that of the *negative* extremity of the *columns*. This was the occasion of the last change which I made in the apparatus, as represented in the *figure*, which part I am now going to describe. Alteration suggested.

On the lead base 19, 19, I fixed two insulating pillars 27, 27, and on these a brass piece 28, at one extremity of which is fixed the *ball* 18, and at the other the machinery for moving the horizontal *silver wire* 21. By this insulation of the parts against which the *bead* and its *silver wire* come to *strike*, I can place them in a moment in communication with either the *ground* or the *negative* extremity of the *column*, by only changing the position of a brass wire 29, hooked to the extremity of the horizontal brass piece. In the position of this *wire*, as represented in the *figure*, the *ball* 18 and the *silver wire* 21 are in communication with the *ground*; and when it is wanted to make them communicate with the *negative* extremity of the *columns*, I have only to take up the movable *wire*, and to lay its end on the projecting brass piece of that extremity. I was surprised to find so little difference of effect between the communication of the *ball* 18 with the *negative* extremity of the *columns*, and with the *ground*, which is a standard between the *negative* and *positive* states of bodies; and upon the whole this kind of observations opens a new and interesting field of researches. Therefore, though I had but a short time to follow these observations, the following journal will show at least the nature of this phenomenon: Addition to the apparatus.

May

			Barom.	Thermom.	Hydrom.	Numb of strik. in 5 min. With the ground.	Numb of strik. in 5 min. With the negat extrem. of the col.
May							
26.	4	P.M.	30·15	71	38¾	3	6
	9		id.	68½	39	2	2
27.	8	A.M.	30·13	61	39½	2	2
	11		30·11	62½	39¼	2	3
28.	1	P.M.	30·30	65	38½	1	2
	3		30 40	66	id.	2	2⅛
29.	7	A.M,	30·55	56	id.	⅛	1
	12		id.	64	37¾	1⅛	2¼
	8	P.M.	30·53	68	37½	⅞	3
30.	7	A.M.	id.	58½	38¼	⅛	1⅛
	1½	P.M.	30·50	66	38	4½	4
	4		id.	69½	38½	4	3¼
	9		id.	67	38½	1¼	2
31.	7	A.M.	id.	60	38¾	1	1⅛
	2	P.M.	30·45	68	id.	4	3
June	9½		30·48	69	id.	2	1⅛
1.	8	A.M.	30·45	63	39½	4	4
	11		30·44	64	39¼	4	4¼
	3	P.M.	30·45	69	37½	1⅛	1½
	9½		id.	69	38	1¼	1⅛
2.	7	A.M.	30·42	62	39	1½	2
	11		id.	65	38½	2½	3
	2	P.M.	30·38	69½	38¼	4	4
3.	6½	A.M.	id.	64	38½	⅛	1¼
	10		30·40	65	id.	2	2
	4½	P.M.	30·38	69	37¾	1⅓	1⅓
4.	8	A.M.	30·45	60½	38½	1½	1⅔
	11		30·40	63	id.	1⅞	2⅛
	2½	P.M.	id.	68	38	2¼	2⅛

Object of the last observations.

In the last days of these observations, I had some reason to suspect, that something had been deranged in the apparatus, but I could not examine it, as I was preparing to leave Windsor for spending the Summer in Devonshire, where I write this paper. However, the defect which I suspected did not interfere with the object of this last series of observations

observations, which principally relates to the *electric state* of the *ground*. This *state* is here compared with that of the extremities of the *column*, which I have called *negative*, though it is sometimes *neutral* comparatively with the *electric state* of the *ambient air*; but it is never *positive*. On the other hand, the *bead* never moves but as *positive* comparatively with the same standard, and it moves the faster, as the *ball* 18 differs more from its *electric state*. Now it is seen in the above observations, that sometimes the *bead* moves faster when the *ball* is in communication with the *ground*, than when it communicates with the extremity of the *column* called *negative*. This is a test of the *electric state* of the *ground* which deserves to be deeply studied, in order to understand it better.

Were I younger, I ought not to publish these experiments and observations in their present state; I should endeavour first, to improve the instrument, in order to meet with more advantage a proper season; then to follow the motion of the *aerial electroscope* more regularly than hitherto I have been able to do, being constantly employed in improving it; and to study the connexion between these motions and the changes in the *electric state* of the *air* near the *ground*, and of the *ground* itself: a course of observations, which is to be followed from the time of the greatest effects of the *column*, to that of their rapid diminution, coinciding with the time when *vegetation*, the greatest terrestrial phenomenon, prevails on all the ground, and in which it thus appears, that the *electric fluid* has some influence. But though it is possible, that I may take up again these observations, I prefer an earlier communication to natural philosophers of the beginning of researches of this class; because at any rate these researches would advance more certainly, should they become the object of many observers, not merely for assembling scattered and unconnected phenomena, but for considering the light that they reflect upon each other, which may help to trace up their real causes. No spontaneous effects can manifest in a more characteristic manner these remote connexions between terrestrial phenomena by common causes, than those offered to our view by the *atmosphere*, in which therefore we must endeavour to extend our knowledge by meteorological

Electric state of the ground and air near it connected with vegetation.

meteorological observations: and as these phenomena have been for a long time one of the principal objects of my attention and study, I purpose to explain in the last *part* of this paper the connexion, that may exist between the indications of the *aerial electroscope*, when properly settled, and many *atmospheric phenomena*, which are daily observed, without being really understood.

Ashfield, near Honiton,
23d August, 1810.

II.

On the Structure and Classification of Seeds. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

I NOW once more trouble you on the subject of seeds, desirous of completing the task assigned to me, and finishing the sketch I began in my last. As in *that* I was careful to confine myself within those *laws*, which are applicable to the interior of the embryo; in this I shall take a different path, and show the various kinds of corculum into which all seeds are divided, and thence the various classes they might form: indicating the interior marks, which would diversify each different class, the very mechanism of which is so various, though so well defined, as to strike the mind with the appearance of a natural method; by *which means* might be established, without any difficulty, an arrangement, which would enable botanists to add them in an appropriate word to the Linnæan or any other classification preferred in giving the description of plants. Certainly, as I before observed, it is strange to give an elaborate description of every part of the exterior of the plant, even the most insignificant, and leave out the most important, the *interior of the seed*; that which is the very *essence* of it (for so the embryo may be called). In our best works of the kind no notice is taken of the heart or *cotyledons*, or of the division so well known to gardeners of leaf seeds.

Various kinds of corculum.

To

To explain this, and other differences, so deeply marked by the hand of nature, I have divided seeds into five classes, to which may be added orders and genera, as many as may be found necessary in future. At present I shall confine myself to a mere sketch of these five divisions, which will be but an outline to be filled up by future observation. Seeds divided into five classes.

That the heart is the laboratory of the seed, I am perfectly persuaded; for in this part begins the *whole work* of nature; from this appears to arise all its mechanical ſtrength; here are concocted, prepared, and perfected, all the various juices: in short, in this part only is seen all the variety of mechanism necessary for these purposes. I shall endeavour to prove this by a description of the various parts that compose it; which, when well dissected, and properly arranged, appear as surprising a piece of work as nature can produce. The heart the laboratory of the seed.

I am not in the least astonished, that physiologists imagined, that in each seed was found the epitome of many trees—for a cursory view of the corculum in a microscope might lead to such a conclusion, from the variety of figure it announces. But we are now too well informed to admit such fables. The miniature tree supposed to be seen in the seed.

Still the mechanism of the corculum is hitherto unknown, at least *undescribed* by any author; and I flatter myself, though I cannot *explain* the *nature* of each secretion performed there, that I shall (as far as the sight can discover) *show its structure.* The present letter will receive additional interest from a discovery I have just made (even since my last letter); for I flatter myself it will complete what my first letter showed; the necessity of abandoning that arrangement of Jussieu, which is founded on the number of cotyledons. For if it can be proved, that there are no plants *without* cotyledons; that what he announced as monocotyledons were *dicotyledons*, and that what he mentioned as dicotyledons *have many*; it muſt of course be confessed, that the arrangement is erroneous, and wants correction. In my former letter I plainly proved, that the primordial leaf in the grasses and palms had been mistaken for the cotyledons. Mechanism of the corculum unknown. Jüssieu's arrangement by the cotyledons erroneous.

I shall soon give a proof of this, not to be controverted, as nature herself will show it; but I may now make a farther assertion, and say, that, *except* in seed-leaves there are few

Recess discovered in a walnut.

few dicotyledons. Dissecting a very unripe walnut, in order to discover the course of its nourishing vessels in the corculum, I perceived a recess I had not before noticed. Desirous of knowing what it contained, I fastened a double magnifier of great power over it, so that I might scrape its interior with a fine lancet. I drew out three diminutive points, which in the solar microscope I soon found to be perfect cotyledons. I then dissected another walnut, and discovered four much larger, which covered the lower part of the corculum like a festooned curtain. These I had before seen, and taken for scales of the kernel, so assured was I, that there could be but *two cotyledons*. Hence the mischief of trusting to any person, or thing, in the *study of nature*, but herself. I then divided the corculum as usual, and found the common cotyledons: in one walnut therefore I found nine, and in above 60 more I discovered from four to seven. I doubt not many more may be seen in an older walnut. I shall now give the picture of the corculum of the walnut, which will better enable the reader to understand the description of the different classes, and of the number of cotyledons that belong to each, with all the rest of its arrangement.

The walnut has from four to nine cotyledons.

Corculum of the walnut.

Plate V, fig. 1, represents the corculum of the walnut when covered by the upper colytedons: *x x* two of the colytedons taken off. Fig. 2, the corculum or heart of the walnut when divided, showing what have always been reckoned cotyledons at *c c*, and the breast with the teats *d d*. Fig. 3. the corculum turned so as to show the back. *f* The recess.

Horse chestnut.

After opening a quantity of walnuts, it may well be imagined, that I was impatient to see whether a recess was to be found in all seeds; and if other seeds had these additional cotyledons. The first I tried was the esculus, which had exercised my patience by constant dissections for a long time, in search of them. But I had now found a clew, and discovered therefore 4; though from the peculiarly uneven formation of its pocket, it was not till after a pretty long search. I must therefore recal the declaration in my last paper, nor shall I ever fail to do so, when I find myself mistaken; seeking

seeking for truth, I may err, but I will never deceive. Since my last discovery, I have been able only to dissect 200 seeds; but these and my former studies amount to above 2000, and will enable me to judge in what class the many-cotyledons are found. I shall now therefore begin with the description of the formation of the corculum of each different class.

1st class. Mammiferæ.

The first is the mammiferous. It includes plants of a very strong and vigorous form and nature, not only in trees and shrubs, but in smaller plants. The oak, beech, elm, horse-chestnut, &c.; the laurel, rose, budlea, &c.; burdock, sun-flower, and many of the order pentandria digynia, that are spreading and vigorous. This seed has a remarkably large heart, into which the juices are conveyed by the nourishing vessels. In the corculum is found that curious part, which resembles the breast of a bitch; with teats (as far as I have been able to discover) numerous according to the strength of the plant. Over the teats are the nourishing vessels, and so much juice do they impart to these curious forms, that the recess is often inundated with the juice that runs through them. What effect this straining may have on the liquid is easy to imagine, and that from crude and harsh it may become both sweet and emollient. It may also be more intimately mixed, and thus form in those spaces gasses suited to the object to be nourished. There is found in this species of heart from 12 to 16 teats, which bleed in three or four places. The recess is discovered at the back of the corculum, and through the middle of it passes that line, which afterward is called the stalk of the plant, and is now only the line of life; and one row of wood vessels, covered by the circular skin of cotyledons, or of that matter which forms them. See Fig. 6. This recess is the place where the cotyledons are mostly formed, and from which they branch, while the primordial leaves proceed from the interior line. This structure plainly proves, that the nourishing vessels are the feeders of both cotyledons and primordial leaves. All the fruits have a heart of this kind, and many cotyledons. Most of the carices and wheat are of this class. It is curious, that in this not only the latter but the former leaves will appear, and burst through the thickness

The seed described.

observed *over* the recess, to show their strength and vigour; this is often seen in the apricot, peach, &c.; which form in this manner two little noseguys. If a strong magnifier be directed to the top of the recess at the back of the corculum, it will show a small aperture, through which the hidrogen enters the heart. This I believe, because before this vessel is seen the heart never cracks in the fire, and the moment it is found the seed explodes.

2d class Foliferæ.

The 2d class I have named foliferous. See fig. 7. Every gardener knows the seed leaf from those seeds that are not so. It is an embryo that rises out of the earth with its cotyledons: though they do not all appear above ground; but those plants which have only two, show themselves growing up with the stem for a little time at least. This class might well be divided into two large orders. The firs with those rising plants that have many cotyledons; and those which have only *two*; but this I shall leave to a future arrangement. The second class then consists of the firs, and a number of plants that are the spontaneous growth of the soil, the pride of the fields, arenarias, stellarias, cinquefoils, euphorbias, beside all running and twining plants. This seed has a remarkably small heart, with a few points, that can hardly be called teats, though they seem to act as such, having the nourishing vessels above them. They have I believe seldom above two cotyledons, though we do know an exception to this rule in the mustard and cress, the former of which has four, the latter six. I mentioned in my last letter, that I would show the difference of growth of a seed-leaf and a common seed; that is, every seed that is not a seed-leaf. Till the end of the second epoch they exactly resemble each other in their manner of growing. The seed-leaf then, instead of shooting out its *primordial leaves*, continues to increase its *cotyledons*, which grow on in size, till they turn to leave the seed, which they do in exactly the same manner as in the other seeds. There is little difference therefore, except that the primordial leaves do not *shoot, till* they have quitted the seed. To this class belong most of those plants, which are found in the class cryptogamia of Linnæus: the lichens, the mosses, the fungi. The fungi I have not however thoroughly ascertained, and leave them

Divisible into two orders.

The seed described.

Difference between the seed leaf and common seed.

them for a future trial; nor have I dissected the seeds of the sea-weeds.

The third class of seeds is a numerous one, and I have called it *The canaliculated.* See fig. 10. It is distinguished by a larger heart, with a curious sweep, which figure the teats follow. The teats are numerous, and have the nourishing vessels above them. This class takes in almost all the papilionaceous, ringent, and many of the cruciform flowers. The formation of the corculum (much as they may differ in each seed) will still be found to have the mark of this class; which is principally a deep furrow beginning with the recess, runing on to the end of the primordial leaves, and lengthening as the embryo increases. I have two or three times found cotyledons in this passage, and I am rather inclined to believe, that farther search will show more, especially in the papilionaceous, which is also distinguished by a curious sheath, that holds that jelly found constantly in the pocket of the seed, and against which the primordial leaves shoot. But I do not conceive, that more than four cotyledons will ever be found. I have never seen more. 3d class Canliculatæ.

The fourth class is the nonmammiferous, and is the one that differs most from the rest; for it has neither *recess, nor teats.* See Pl. VI, fig. 1 and 2. The palms, and grasses, are included in this; beside many odd plants, which it would be useless in such a sketch to mention. The distinguishing marks of this class are the *cotyledons* proceeding from the upper end of the corculum, instead of the usual place; this was the reason, that in the grasses botanists overlooked them; took the *primordial leaf* for the *cotyledons,* and named them monocotyledonous. But had they dissected *the interior,* they would have found, that they are placed (with respect to the primordial leaf) *exactly* as in every other plant; both rising and branching from the same apparent source. This is sufficient to prove, that these little leaves (always given in those excellent drawings of Sowerby) are really the *cotyledons* of *the grasses*; and that they have always either two or four, as well as the palms. The class is also easily known by having the stalks running through the corculum without impediment; and the nourishing vessels protruding on one side of the heart only, which has been the 4th class Nonmammiferous.

cause of many mistakes concerning the radicle, which I mentioned in my last. The false grasses, (such as the *cyperus, scirpus, carex,* &c.) belong to the first, as well as wheat and rye; but barley, oats, &c. to this. When my plan is more perfected *(if approved)* I hope to give a list, that will more exactly point out the arrangement.

5th class. Mixed, or compound.

The last class I have called the mixed or compound seeds. See Pl. VI, fig. 3. It includes most of the water plants, the spice, coffee, and some cotton plants. I have not yet been able to acquire foreign seeds sufficient to enable me to arrange it with the perspicuity I would wish; but it has notwithstanding some striking features, fully capable of marking and distinguishing it from the other classes: for it has the large and prominent heart of the first class, with the seed leaf of the second; it has many teats, and a roomy recess, for the formation of the cotyledons. I have no doubt, that many seeds I am yet unacquainted with will rank in this class. The bladder tree appertains to it, and a curious plant brought me by a gentleman from the East Indies, who was one of those engaged in the trigonometrical survey there, and who found it in the wildest part of the peninsula, that few but themselves ever crossed. I have not been able to procure Rumphius, to seek it there; and can find it only in Gerard, who calls it "arboris lanifera siliqua." Supposing it little known, I have selected it as an example of this peculiarly formed *corculum*, well marking the class, and shall describe the plant also. It has a pod six inches long, two and a half wide, full of the most beautiful cotton, weighing nearly a quarter of a pound, and having within the seed vessel a number of triangular black seeds, rounded at the edges. In dissecting this seed, a large *heart* is found, rather larger in proportion than in the first class, and having two seed leaves of great length, curled up very thick, and the intermediate part of the seed filled with a substance like flower. On stretching the cotyledons, they measured near an inch and half, and in some seeds I have found two cotyledons *above:* and in most seeds of this class there are from *four* to *six*. This tree is a large one, and has leaves very long and slender; the outside rind is thick and spongy. The flowers I have not seen, nor have I received

I received any description of them; but the cotton resembles silk, and is more beautiful than that the silk worm spins. It is said to grow also in Bantam, and to be much valued. To complete the account of the corculum and of this fifth class I shall only mention, that it has from 14 to 18 teats, with very large nourishing vessels: the long cotyledons almost wholly fill the seeds in general; and it appears to me an additional proof, that they contrive to grow as long as their room and time will admit; for seldom can there be found seeds showing a regular number of cotyledons: the longer they remain in the seed vessels, the more there are; and in this last class, the longer they are.

Number of cotyledons never regular.

I shall now give a few hints to those botanists that wish to dissect their own plants, and to judge for themselves. Patience and habit are every thing: perhaps in no particular does practice repay so amply as in this. The hand grows more delicate in the touch; and the eye so very much improves in sight, that what at first cannot be seen distinctly, with a good magnifier, will soon become plain to the naked eye. The habit of dissecting with the mouth likewise all botanists should endeavour to learn, for no instrument can act like it, or so thoroughly divest the seed of all superfluous parts, and prepare it for the microscope.

Hints on dissecting plants.

As to the rules for distinguishing these classes, without obliging any person to repair to the solar or other powerful microscope: the first class is easily known by a small magnifier, but the second requires some art. They are generally remarkably small seeds; press them between the nails of your thumb, beginning the pressure at the corculum end, and the whole embryo will slip out *heart and all*; you have then only to divide it with a fine lancet. The third class must not be so tried, but lay it straight on your seed hammer; and pressing a flat knife on it, pass your lancet between, and it will always divide it *exactly* as it should do, showing the two principal vessels in a manner that will teach much, for this class of seeds is one of the best to begin dissection by, as there is no confusion in the arrangement of the vessels. They are at such a distance from each other, that it is hardly possible to mistake them. I have drawings of a large size of many of this class, which are of

Rules for distinguishing the classes.

of great use in showing the formation and habit of a seed, and teach more (if well studied) than any other: the 4th and 5th classes are large enough to be dissected with the usual instruments. But for the diminutive seeds larger powers are required, as the powdered lichens, fungi, mosses of the smallest kind, &c. It is best to keep these till *very* ripe, then place the seeds in the several sliders of the solar or double microscope, and you will always find two or three opened sufficiently by the heat and light, to draw the figure of the interior, if extremely magnified.

Cambium or albumen.

I shall now conclude the present letter with the explanation of a term that has long demanded attention, particularly on the subject of seeds, which it concerns greatly. I mean the word albumen or cambium, a matter found wherever new wood is to be created. Duhamel calls it *cambium*; Mirbel "la substance organisatrice," and gives this description of it: "Soit que les fluides y développent "par leur impulsion les cellules, et les tubes; soit qu'une "puissance inconnue, y agisse seule et y détermine ces "développements; soit, comme il est probable, que ces "deux causes combinées, y agissent de concert pour chan-"ger en tissu membraneux la substance organisatrice, "&c." I cannot think this is described with his usual perspicuity, for he does not even show what it really is. Mr. Knight makes it much more plain, but thinks it proceeds from the bark. Much as I have profited by his remarks, which always carry with them the conviction, that he has *deeply studied the subject*, I cannot agree with him in this opinion. I have perpetually seen it grow on the dry piece of a seed vessel, which I have placed *within* the *graft* for the purpose; in the same manner I have put the edge of a knife, and a diminutive piece of muslin, and found the cambium growing on them, as on the wood, and bark. Now whence does this substance proceed? from the juices of the plant alone, from the mixture of the sap with the blood of the plant, resting on the part, and there forming as a crystal, since like a crystal it is the produce of the joint juices. But it is very different from the jelly found in the pocket, which also has been improperly called albumen. I shall now give an account of it, describing its appearance

pearance in the solar microscope. It is all composed of extremely diminutive netted bags, of thick juice, without any vessels—in short, it is the first formation of the pabulum or softer part of the wood; and when ready prepared for the sap vessels, they shoot their way through this soft substance. In a graft, which I have repeatedly tied up again, *before* the vessels had begun to appear, and when I reopened it, I found them and the wood perfectly complete. I have taken this matter from a graft, from a fresh budded plant; from the *interior of a seed*, and sometimes from the shooting of the fresh line of the wood, but this is generally too hasty a performance to profit by; the fresh wound of a tree is the best way of getting it (next to a graft) if well preserved from the air, and in a fortnight plenty will be found. But the specimen must be quickly taken, or the wood vessels will shoot. This is the true cambium, the softer part of the wood, before the sap vessels shoot. But I must notice, that the bark is not made in the same manner: it is formed all at once, soft and hard; the vessels shoot, while the rest is forming. Mr. Knight very properly observes, that in a graft the fresh wood always resembles exactly the wood of the graft, and not the stock.

How obtainable.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

The five classes into which I have divided the seeds.

Classes of seeds.

Common seed, or first class.

Mammiferous, or teat-bearing. See walnut, Pl. V, figs. 1, 2, 3; apricot, figs. 4, 5, 6.	Oak, elm, beech, horse-chestnut, &c.; rose, laurel, budlea, &c.; burdock, sunflower, and many other compound flowers.

Second class.

Leaf-bearing or foliferous. See figs. 8 and 9, showing the whole embryo, when forced out of the seed in the manner described above; and fig. 7, the heart, or corculum, alone.	Firs and spontaneous plants of the soil, as arenarias, stellarias, potentillas, euphorbias, and many of the running plants, &c.

Third

Third class.

Caniculated, or channelled, so called, from a channel, which begins within the recess, and runs on beyond the primordial leaf. See fig. 10, the upper part representing the corculum; the lower, the whole of the embryo together.	A numerous class, containing most of the papilionaceous, cruciform, and labiate plants.

Fourth class.

Nonmammiferous, having no teats, and no recess: distinguished also by having the primordial leaves as well as the cotyledons at the head of the corculum. See Pl. VI, figs. 1 and 2.	Grasses and palms.

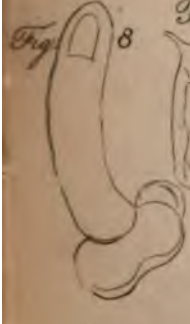
Fifth class.

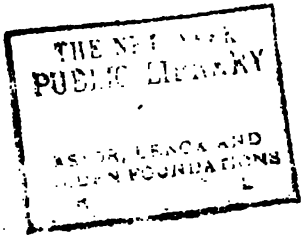
Compound or mixed seed. See Pl. VI, fig. 3.	Nymphea, coffee, some spices, and cotton tree.

Fig. 6 is merely to show the manner in which the stalk, *n*, runs through the corculum; the primordial leaves, *e e*, being within; the cotyledons, *c c*, shooting from the outward cylinder.

In all the figures the same letters of reference are used. *a*, the line of life, or impregnating duct. *b*, nourishing vessels. *c*, cotyledons. *d*, the breast and teats. *e*, primordial leaves. *f*, the recess. *n*, the stalk.

Mr. Ibbetson on the Structure & Classification of Seeds.

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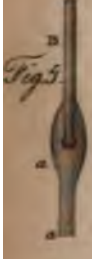
Mr. Ibbetson on the Structure & Classification of Seeds.

Fig. 4

Mr. Richardson's method of raising large Stones out of the Earth.



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III.

Method of ascertaining the Value of Growing Timber Trees, at different and distant Periods of Time. By Mr. CHARLES WAISTELL, *of High Holborn.*

(Continued from p. 144.*)*

Observations on the Tables respecting the Thinning of Woods, and their Produce.

MR. Salmon is the only person I know of, who has given a general rule for thinning plantations. But as I conceive his distance of one fifth of their height would leave oaks too close, especially after they had acquired a sufficient length of stem, I have calculated both on his plan, which is proper for fir trees, and also at greater distances.

One fifth of their height too close for oaks, not for firs.

The preceding Tables VI, VII, and VIII, are calculated on a supposition, that the trees are never suffered to stand nearer, on an average, than one fourth of their height; and although the quantities of timber thinned out and left standing on the ground at that distance, at the end of 60 years, is only two thirds of the quantity according to Mr. Salmon's distance, yet I suppose it will be generally thought an ample produce, and sufficiently encouraging.

Tables calculated at one fourth.

According to Table VI, which is calculated for oaks, the first thinning is at sixteen years old, and the second at twenty; but it is the advice of an eminent planter, (Mr. Pontey,) to begin thinning at about thirteen years old, according to the state of the trees, and to cut out about 150 poles per acre annually, for the next seven years. Without putting any value upon the thinnings before 20 years old, we find that at the 20th and 24th years, the thinnings measure 945 feet, the value of which, at a low estimate, will be sufficient to repay the rent and taxes of ground of a moderate quality, with the expense of plants, planting, and after-management, calculated at 5 per cent compound interest.

Thinning of oaks.

When

Profit of thinnings after 28 years.

When 28 years old, and at the end of every fourth year following, up to 120, the trees to be cut out of an acre will measure from 495 to 550 feet; but say 500, at 4*s.* a foot, on an average, including the value of the bark; this gives 100*l.*; which sum, divided by 4, leaves 25*l.* for the produce per acre per annum. This deserves the consideration of those who are inclined to convert young woods into coppices, without leaving a reasonable number of standards.

It may however be said, that, as the trees cut out in thinning plantations are the bad thrivers and underlings, their contents will be less than the average; but, if we take their value at one half the above estimate, that is, after the rate of 12*l.* 10*s.* per acre per annum at 28 years of age and upwards, even this produce must be thought ample, together with the value of the trees left standing.

Table for oaks.

Table VI was constructed chiefly with a view to oaks, their annual increase in circumference varying from $\frac{1}{4}$ of an inch to $1\frac{1}{4}$ inch, the medium of which is 1 inch.

Tables for fast growing trees.

Table VII and VIII were calculated for ash, elm, sycamore, firs, poplars, and other woods of swift growth, their increase in circumference being generally from $1\frac{1}{2}$ to 2 inches annually. If ash trees be found to increase after the rates of Table VII, or VIII, they must be exceedingly profitable, at the high prices now given for that timber. Many other observations might be made on Tables VII and VIII, but these will readily occur to persons interested in quick growing trees.

Differences in the tables.

An acre of trees, increasing after the rate of Table VI, produces in 64 years little more than half the number of feet, that another acre produces, which increases after the rate of Table VII; and little more than one third of another, increasing after the rate of Table VIII, in the same time.

Advantage of thick planting.

In planting with a view to profit, the first object is a long, straight, and clear stem. This is most certainly and speedily obtained by thick planting at first, and not thinning too soon. A kind of competition among the trees is thereby occasioned, each struggling, as it were, to outgrow its neighbour, in search of light, heat, air, and moisture.

This

This competition must, however, be judiciously moderated by timely thinning; always keeping the trees sufficiently strong in the stem. If they be suffered to stand some years too near each other, their stems will become weak, and bend under their small tops when thinned. Where this has taken place in only a small degree, they will make but little progress for some years afterward. and timely thinning.

By the time the trees have advanced to 24 or 30 feet high this competition should cease, if they are intended to be cut down at or before 60 years of age; and they should then be encouraged to extend their tops more in width than in height, strong side branches being apparently quite as conducive as the leading shoot, to the vigorous growth of the bole below them. After this period, the best rule for thinning will probably be, to leave a clear space around the top of each tree, in which the branches may extend themselves without obstruction. A tree the top of which is 20 feet diameter, receives four times the benefit from air, rain, and dew, that another does, the top of which is only 10 feet diameter. After a time strong side branches necessary.

The trees in the interior of young woods are smaller in their boles than the exterior trees. And in a fine oak wood, of about 40 acres, divided into squares by several avenues or ridings crossing each other at right angles, I observed the rows of trees next the avenues much thicker in their boles than the trees in the interior of the squares; owing, no doubt, to their having more and larger branches in consequence of their having more room, although it is only on one side. Outer trees of a wood largest.

Being too parsimonious of ground seems to me a great and very general errour. If the same number of trees of 32 feet high and upwards, in Table VI, were allowed the space of two acres instead of one; and, in consequence of their standing thinner, were to increase annually only the fiftieth part of an inch more in girt, than they would do if they stood on one acre, this small additional increase in girt would pay an ample rent for the additional acre. Advantage of thin woods.

In the year 1791 a paper of Observations on the Propagation and Management of Oak Trees in general, but more particularly

particularly applying to his Majesty's New Forest in Hampshire, was published by T. Nichols, Purveyor of the Navy for Portsmouth Dock-yard.

In this paper it is said, that "there are to be seen in many parts of the forest from 40 to 50 fine oaks standing on an acre, that will measure one with another two loads a tree."

Injury from neglecting to thin.

"Several woods in the forest are almost ruined for want of thinning, and it's being done at proper times; particularly the enclosures that were made in the year 1700;—these were originally well planted, and great numbers of trees brought up in them, which now remain so close together, that they are nearly stagnated, particularly in Salisbury, Trench, Brimley Coppice, and Woodfidley; and, although it is 90 years since they were planted, the trees will not measure, one with another, above six or seven feet a tree; whereas, if the business of thinning had been done as it ought, the remaining trees (after drawing much useful timber) would by this time have been of a size nearly fit for naval uses; as in some of the woods, that were planted at the same time, the trees which have had room to expand, and a free air admitted to them, will measure from 70 to 80 feet."

Observations on the Growth of Timber.

Timber grows thickest on the side next the sun.

The rings observable in the transverse section of a tree at its but-end are the same in number as the years of its age; an additional ring being produced annually, in consequence of the annual rising of the sap. The rings are nearly concentric in trees that have grown in the interior of close shady woods, but eccentric in others, being of different breadths on the northern and southern sides of such as have grown single, or in any other situation, where their boles have been much exposed to the rays of the sun. This difference is occasioned by the different degrees of heat, to which the opposite sides of the boles of trees are exposed. And, indeed, we find these rings are always broadest on that side of the bole or stem most warmed by the sun. Hence we see the utility of exposing their boles as much as

as possible to its rays*. It is often seen in the stumps of trees that have stood single, that they have grown nearly twice as fast on the southern side as on the northern, their pith being so much nearer to the northern side.

but the wood is not so heavy.

It is, however, to be remarked, that the wood from that side of a tree, which has grown the slowest, is heavier than from the opposite side, which has grown the fastest; and it is probably stronger in the same degree.

Plantation of southern slopes.

It may be worth the consideration of those, who have southern hangs or declivities to plant, whether to plant, or rather leave the trees in thinning, in double rows in lines running east and west, at about fourteen or sixteen feet distance, and the double rows at about thirty-six feet distance, less or more, according as the declivity is more or less, in order that their boles may receive the greatest possible benefit from the direct rays of the sun.

Facts respecting the action of the sun on trees wanted.

No doubt many gentlemen are in possession of facts, that would in some degree ascertain how much faster the boles of trees swell, that stand exposed to receive the full benefit of the warmth of the sun, than those that are either partially or constantly in the shade. To make these facts known would materially benefit planters; for I am fully persuaded, that there are but few persons apprised of the magnitude of the power of the sun's rays upon the boles of trees in causing them to swell.

Of the most profitable Length of Boles of Trees.

Cautions on pruning forest trees.

We rarely see timber trees pruned, and still more rarely do we see the pruning performed in a judicious manner. This business should commence early, never suffering the branches on the intended stem or bole to grow to a large size, in order that, when cut off, the wounds may be small and soon healed. Those who want directions for performing the operation may think well to consult Mr. Pontey's Forest Pruner. There are, however, divers opinions as to the

* On a hot day in the middle of May I have observed the mercury in the thermometer to rise and fall from twelve to sixteen degrees, on hanging it alternately on the sunny and shady sides of the same tree, between the hours of two and five o'clock, at which time of the day the heat is generally the greatest. Difference of heat on the two sides of a tree.

most

most profitable height, to which trees ought to be pruned, and the instruments most proper for pruning; some persons objecting to the use of the saw, unless afterward smoothed by the knife; and not a few objecting to pruning in any way; the consequence of which is, that we often find trees that stand single, particularly oaks, with boles not more than six or eight feet high, but with wide spreading bushy tops, fit only for the fire. The shade and drip of one such tree is sometimes found to do more injury than four well-trained trees, and perhaps it is not of half the value of one of them. On the contrary, trees in close plantations are often suffered to stand so much too thick as to destroy each other's branches, excepting only a few small ones near their tops; and not unfrequently we see tall elms trimmed up to within a few feet of their summits—it is certain, that such trees must swell very slowly in their boles; for we find in woods where the trees are all of the same age, that those with the largest tops have generally the thickest boles.

Proper length of bole.

There is no doubt a medium length of bole for different kinds of trees on different soils, that will be found productive of more timber, or timber of more value, than boles that are much longer, or much shorter. And although we may not be able previously to decide with certainty what that exact length of bole is, in any kind of trees, on any soil, which will eventually prove most profitable, yet it is deserving of investigation, if we can thereby approach with certainty to within a few feet of the exact point. It is certainly a matter of too much importance to be left, as it generally is, to each individual woodman to decide upon, according to his own vague opinion. I shall, therefore, take the liberty of stating by what steps I have endeavoured to approximate towards the most profitable lengths of boles of trees of different rates of growth, that are not intended to stand beyond the age of sixty years.

In the preceding tables the trees are supposed to be measured to the top of the leading shoot, but in the following tables only to the height of their boles of 24, 32, and 40 feet.

TABLES

TABLES *showing the Increase of* BOLES *of* TREES *of different Lengths.*

Table 10. On the length of boles.

If a tree has increased twelve inches in height and one in circumference annually, until it is twenty-four years old, it will then be twenty-four feet high, and three inches girt at twelve feet high; and supposing, that in process of time this tree is pruned up so as to leave the bole twenty-four feet high clear of branches, and that it continues increasing one inch in circumference annually; the rate per cent per annum of its increase will be as under, exclusive of the increase of timber in its top and lateral branches.

TABLE X.

Years old.	Girt	Contents			Years old.	Girt	Contents.			One year's increase.			Increase per cent per ann.
	in.	ff.	in.	p.		in.	ft.	in.	p	ft.	in.	p.	
24	3	1	6	0	25	3¼	1	9	1	0	3	1	17·1
28	4	2	8	0	29	4¼	3	0	1	0	4	1	12·7
32	5	4	2	0	33	5¼	4	7	1	0	5	1	10·1
36	6	6	0	0	37	6¼	6	6	1	0	6	1	8·4
40	7	8	2	0	41	7¼	8	9	1	0	7	1	7·2
44	8	10	8	0	45	8¼	11	4	1	0	8	1	6·3
48	9	13	6	0	49	9¼	14	3	1	0	9	1	5·6
52	10	16	8	0	53	10¼	17	6	1	0	10	1	5·04
56	11	20	2	0	57	11¼	21	1	1	0	11	1	4·5
60	12	24	0	0	61	12¼	25	0	1	1	0	1	4·1
64	13	28	2	0	65	13¼	29	3	1	1	1	1	3·8
68	14	32	8	0	69	14¼	33	10	1	1	2	1	3·5
72	15	37	6	0	73	15¼	38	9	1	1	3	1	3·3
76	16	42	8	0	77	16¼	44	0	1	1	4	1	3·1
80	17	48	2	0	81	17¼	49	7	1	1	5	1	2·9
84	18	54	0	0	85	18¼	55	6	1	1	6	1	2·7
88	19	60	2	0	89	19¼	61	9	1	1	7	1	2·6
92	20	66	8	0	93	20¼	68	4	1	1	8	1	2·5
96	21	73	6	0	97	21¼	75	3	1	1	9	1	2·3
100	22	80	8	0	101	22¼	82	6	1	1	10	1	2·2
120	27	121	6	0	121	27¼	123	9	1	2	3	1	1·8
140	32	170	8	0	141	32¼	173	4	1	2	8	1	1·5
160	37	228	2	0	161	37¼	231	3	1	3	1	1	1·3

But

Table 11. On the length of boles.

But if a tree increase 12 inches in height and one inch in circumference annually, until it is 32 feet high, and in process of time the bole be pruned up to this height, the rate per cent per annum of the increase of this bole will be as under, exclusive of the increase in its top and lateral branches.

TABLE XI.

Years old	Girt.	Contents.			Years old.	Girt.	Contents.			One year's increase.			Increase per cent per ann.
	in	ft.	in.	pts.		inch.	ft.	in.	pts.	ft.	in.	pts.	
32	4	3	6	8	33	4 1/2	4	0	2	0	5	6	12·9
36	5	5	6	8	37	5 1/2	6	1	6	0	6	10	10·25
40	6	8	0	0	41	6 1/2	8	8	2	0	8	2	8·5
44	7	10	10	8	45	7 1/2	11	8	2	0	9	6	7·3
48	8	14	2	8	49	8 1/2	15	1	6	0	10	10	6·3
52	9	18	0	0	53	9 1/2	19	0	2	1	0	2	5·6
56	10	22	2	8	57	10 1/2	23	4	2	1	1	6	5·06
60	11	26	10	8	61	11 1/2	28	1	6	1	2	10	4·59
64	12	32	0	0	65	12 1/2	33	4	2	1	4	2	4·2
68	13	37	6	8	69	13 1/2	39	0	2	1	5	6	3·88
72	14	43	6	8	73	14 1/2	45	1	6	1	6	10	3·6
76	15	50	0	0	77	15 1/2	51	8	2	1	8	2	3·36
80	16	56	10	8	81	16 1/2	58	8	2	1	9	6	3·1
100	21	98	0	0	101	21 1/2	100	4	2	2	4	2	2·39
120	26	150	2	8	121	26 1/2	153	1	5	2	10	9	1·92

Table 12. On the length of boles.

But if a tree increase 12 inches in height and one inch in circumference annually, until it is 40 feet high, and in process of time the bole be pruned up to this height, the rate per cent per annum of the increase of this bole will be as under, exclusive of the increase in its top and lateral branches.

TABLE

TABLE XII.

Years old.	Girt.	Contents.			Years old.	Girt.	Contents.			One year's increase			Increase per cent per annn.
	inch	ft	in.	pts.		inch.	ft.	in.	pts.	ft.	in.	pts.	
40	5	6	11	4	41	$5\frac{1}{4}$	7	7	10	0	8	6	10·2
44	6	10	0	0	45	$6\frac{1}{4}$	10	10	2	0	10	2	8·47
48	7	13	7	4	49	$7\frac{1}{4}$	14	7	2	0	11	10	7·2
52	8	17	9	4	53	$8\frac{1}{4}$	18	10	10	1	1	6	6·3
56	9	22	6	0	57	$9\frac{1}{4}$	23	9	2	1	3	2	5·6
60	10	27	9	4	61	$10\frac{1}{4}$	29	2	2	1	4	10	5·05
64	11	33	7	4	65	$11\frac{1}{4}$	35	1	10	1	6	6	4·58
68	12	40	0	0	69	$12\frac{1}{4}$	41	8	2	1	8	2	4·2
72	13	46	11	4	73	$13\frac{1}{4}$	48	9	2	1	9	10	3·87
76	14	54	5	4	77	$14\frac{1}{4}$	56	4	10	1	11	6	3·59
80	15	62	6	0	81	$15\frac{1}{4}$	64	7	2	2	1	2	3·35
100	20	111	1	4	101	$20\frac{1}{4}$	113	10	10	2	9	6	2·51
120	25	173	7	4	121	$25\frac{1}{4}$	177	1	2	3	5	10	2·00

(To be concluded in our next.)

IV.

Demonstration of a curious Numerical Proposition, by Mr. P. BARLOW, *of the Royal Military Academy, Woolwich.*

PROPOSITION.

THE *equation*

$$x^n + y^n = z^n$$

No power but the square divisible into two of the same denomination:

is always impossible either IN INTEGERS *or* FRACTIONS, *for every value of* n *greater than* 2.

This theorem is one of the most interesting in the theory of numbers, both on account of its simplicity and generality, and the celebrity of those mathematicians, who have attempted its demonstration. The theorem itself is due to Fermat, who first proposed it as a challenge to all the Eng-

a proposition of Fermat's,

lish mathematicians of his time; he also introduces it in a note at page 61 of his edition of Diophantus in the following terms.

"*Cubum autem in duos cubos, aut quadrato quadratum in duos quadrato quadratos, et generaliter nullam in infinitum ultra quadratum potestatem in duos ejusdem nominis fas est dividere, cujus rei demonstrationem mirabilem sane detexi. Hanc marginis exiguitas non caperet.*"

who never published the demonstration.

From which it appears, that he was himself in possession of the demonstration *, though it was never published, nor has any mathematician since his time been able to restore it, notwithstanding they have succeeded in demonstrating many other of his propositions.

Euler demonstrated it in two cases,

Euler was, I believe, the first who undertook this task, and succeeded in demonstrating the impossibility in the two cases $n = 3$, and $n = 4$; that is, that the equations $x^3 \pm y^3 = z^3$ and $x^4 \pm y^4 = z^4$ are impossible, and the same two cases have also been demonstrated upon similar principles by Waring, in his *Meditationes Algebraicæ*, and by Legendre, in his *Essai sur la Théorie des Nombres*, the latter author concluding his chapter by the following remark.

as did Waring and Legendre.

"*Nous avons démontré dans ce paragraphe, que l'équation* $x^3 \pm y^3 = z^3$ *est impossible, ainsi l'équation* $x^4 \pm y^4 = z^2$, *et à plus fort raison* $x^4 \pm y^4 = z^4$. *Fermat a assuré de plus* (*Ed. de Dioph. page* 61) *que l'équation* $x^n \pm y^n = z^n$, *est généralement impossible, lorsque n suppasse* 2; *mais cet proposition, passé le cas* $n = 4$, *est du nombre de celles que restent à démontrer, et pour lesquelles les méthodes que nous*

Mistake in a note to the translation of Euler's Algebra.

* I ought here to correct an errour, that I fell into in one of the notes to the second English edition of Euler's Algebra, where in mentioning this theorem I have said, "and the truth of it still rests on no other foundation than the bare assertion of Fermat, who probably had never demonstrated it himself." I was led into this expression by writing the note in question from memory only, not recollecting at the same time the concluding part of his sentence, where he so positively asserts his being in possession of the demonstration: and as it was by no means my intention to impeach the veracity of this distinguished geometer, I ought in justice to his memory to correct the mistake.

venons

venons d'exposer paroissent insuffisantes." (Page 410, Essai sur la Théorie des Nombres.)

This therefore being the state of the proposition, the following general demonstration, for every possible integer value of n, greater than 2, will not, it is presumed, be unacceptable to the lovers of this interesting branch of analysis; before entering upon which, however, at length, it will be proper to make a few preliminary observations on the general equation, in order to render the demonstration as simple and concise as possible.

Observations on the general equation.

1. In demonstrating the impossibility of the equation $x^n \pm y^n = z^n$, it will be sufficient to consider n as a prime number. For suppose n be not a prime, but equal to the product of two or more prime factors, as $n = p\,q$, then the equation becomes $x^{pq} \pm y^{pq} = z^{pq} = (x^p)^q \pm (y^p)^q = (z^p)^q$, being a similar equation, in which the power q is a prime number; and, therefore, if the equation be possible when n is a composite number, it is also possible for a prime power; and conversely, if the equation be impossible when the power is a prime, it is also impossible for every composite power; we shall therefore in what follows consider n as a prime number.

2. We may always suppose x, y, and z as prime to each other; for it is evident, in the first place, that two of these numbers cannot contain a common divisor, unless the third contains the same. Suppose, for example, that x^n and y^n contained any common divisor, as φ, and that z^n did not contain the same, then, in the equation $x^n \pm y^n = z^n$, we should have $x^n \pm y^n$ divisible by φ, but the equal quantity z^n not divisible by it, which is absurd; and the same may be shown if any other two of these quantities are supposed to have a common divisor which the third has not. And if they have all three the same common divisor, as $x = \varphi\dot{x}$, $y = \varphi\dot{y}$, and $z = \varphi\dot{z}$, then the equation becomes $\varphi^n \dot{x}^n \pm \varphi^n \dot{y}^n = \varphi^n \dot{z}^n$, or, dividing by the greatest common divisor, $\dot{x}^n \pm \dot{y}^n = \dot{z}^n$: if, therefore, the equation $x^n \pm y^n = z^n$ be possible, when x, y, and z, have a common divisor, it is also possible after being divided by that common divisor,

and in which latter equation the three resulting quantities, $\dot{x}$, $\dot{y}$, and $\dot{z}$, are prime to each other; and, conversely, if the latter be impossible, the former is impossible also; we shall therefore only consider the case in which x, y, and z, are prime amongst themselves.

3. It will be sufficient to consider the ambiguous sign $\pm$ under either of its forms $+$ or $-$: for if the equation $x^n + y^n = z^n$ be possible, so also is the equation $x^n - y^n = z^n$; and if the equation be impossible under the latter form, it is likewise impossible under the former.

We shall therefore limit our demonstration to the equation $x^n - y^n = z^n$, in which n is a prime number, and x, y, and z, numbers prime to each other: the impossibility of which, from what is shown above, involves with it the impossibility of the general equation $x^n \pm y^n = z^n$, when x, y, and z, are any numbers whatever, and n any number except 2, or some power of 2. Now with regard to $n = 2$, we know, that the equation is not impossible, and the case of $n = 4$ has been demonstrated to be impossible by Euler, and others; and this latter case involves that of every higher power of 2, thus $x^8 \pm y^8 = z^8 = (x^2)^4 \pm (y^2)^4 = (z^2)^4$; which being impossible in the latter form, is necessarily so in the former: and in the same manner, the impossibility of the equation for any higher power of 2, may be shown to be involved in that of $n = 4$: it is evident, therefore, that our equation, together with that of $n = 4$, involves every possible value of n greater than 2.

Lemma 1.

Lemma 1. If there be two fractions, as $\frac{a}{A}$, and $\frac{b}{B}$, each in its lowest terms, and of which the denominator of the one contains any factor not common with the denominator of the other; then I say, that neither the sum nor difference of those fractions can be equal to a complete integer number.

Let $\frac{a}{A}$ and $\frac{b}{B}$ be any two fractions in their lowest terms, so that a is prime to A, and b prime to B, also suppose B

to

to contain a factor t, as $B = tB'$, which is not contained in A; then I say, that $\frac{a}{A} \pm \frac{b}{tB'} = n$ an integer, is impossible.

For $\frac{a}{A} \pm \frac{b}{tB'} = \frac{atB' \pm bA}{AtB}$ which cannot be an integer, because if it were, the numerator $atB' + bA$ would be divisible by the denominator AtB', and consequently by any one of the factors of that denominator, as t; but the numerator $atB' + bA$ is not divisible by t, for since the first term atB' is divisible by t, the second term bA must also be divisible by t, if the whole quantity was so; but bA is not divisible by t, (Euclid. 26, 7) because both b and A are prime to t by the supposition; since, then, atB' is divisible by t, but bA not divisible by it, therefore the whole quantity $atB' \pm bA$ is not divisible by t, and consequently $\frac{atB' \pm bA}{AtB'}$ or $\frac{a}{A} \pm \frac{b}{tB}$ cannot be equal to an integer. Q. E. D.

Cor. In the same manner it may be shewn, if there be any number of fractions, each in their lowest terms, as $\frac{a}{A}, \frac{b}{B}, \frac{c}{C}$, &c., and of which one of the denominators contain a factor that is not common to all the other denominators, that neither the sum, nor difference of those fractions, any how combined, can form an integer; that is, $\frac{a}{A} \pm \frac{b}{B} \pm \frac{c}{C} \pm \&\, \frac{q}{Q} = n$ an integer, is impossible.

Lemma 2.

If any power of a number A, as A^n, be divisible by any other number r, *once*, and after that, neither by r, nor by any factor of r, then will r itself be a complete n th power. Lemma 2.

First if A be a prime number, then A^n can only be divided by A, or some power of A, that is r must be some power of A; but if A^n be divided by any power of A less than A^n, it is evident that the quotient will still be divisible by

by A; but by the supposition the quotient is not again divisible, either by r, or by any factor of r, therefore r must in this case be equal to A^n.

Again, if A be not a prime number; let it be resolved into its prime factors, as $A = a\ b\ c$, &c., or $A^n = a^n\ b^n\ c^n$, &c. Now if $A^n = a^n\ b^n\ c^n$, &c., be divided by any power of a less than a^n, the quotient will be again divisible by a; and in the same manner if it be divided by any power of b less than b^n, the quotient will still be divisible by b, and so on of any other divisor that is not a complete nth power; and, therefore, conversely, if A^n be divisible by any other number r, *once*, and after that, neither by r, nor by any factor of r, then must r itself be a complete nth power.

Q. E. D.

Lemma 3.

Lemma 3. In the expanded form of the binomial $(p + q)^n$, when n is a prime number, each of the coefficients, except those of the first and last terms, are divisible by n.

For each of the coefficients is of the form

$$\frac{n \cdot (n-1) \cdot (n-2) \text{ &c. } (n-r)}{1 \cdot 2 \cdot 3 \text{ &c. } r+1}$$

which is always an integer, from the nature of the binomial; and since n is a prime number it is not divisible by any of the factors in the denominator, which are all less than n, except in the coefficient of the last term, which does not enter into our consideration, the coefficients of the first and last term being excepted in the proposition.

Since then

$$\frac{n \cdot (n-1) \cdot (n-2) \text{ &c. } n-r}{1 \cdot 2 \cdot 3 \text{ &c. } r+1}$$

is an integer, and n is prime to all the factors in the denominator, therefore,

$$\frac{(n-1) \cdot (n-2) \text{ &c. } (n-r)}{2 \cdot 3 \text{ &c. } r+1} = e$$

is also an integer, and consequently

$$\frac{n \cdot (n-1) \cdot (n-2) \text{ &c. } (n-r)}{1 \cdot 2 \cdot 3 \text{ &c. } (r+1)} = n\,e;$$

that

that is, each of the coefficients, except those of the first and last term, is of the form $n\,e$, and is therefore divisible by n.

Q. E. D.

Cor. We may therefore in all cases, where brevity requires it, write Corollary.

$(p \pm q)^n = p^n \pm n p^{n-1} q + n\,a\,p^{n-2} q^2 \pm n\,b\,p^{n-3} q^3$, &c. $\pm n\,p\,q^{n-1} \pm q^n$, n being an integer prime number.

Proposition 1.

If the equation $x^n - y^n = z^n$ be possible, then one of the four following conditions must obtain; viz., Proposition 1.

1st. $\begin{cases} x - y = r^n \\ x - z = s^n \\ y + z = t^n \end{cases}$ 2d. $\begin{cases} x - y = n^{n-1} r^n \\ x - z = s^n \\ y + z = t^n \end{cases}$

3d. $\begin{cases} x - y = r^n \\ x - z = n^{n-1} s^n \\ y + z = t^n \end{cases}$ 4th. $\begin{cases} x - y = r^n \\ x - z = s^n \\ y + z = n^{n-1} t^n \end{cases}$

where r, s, and t, may represent any numbers whatever, indicating only, that $(x - y)$, $(x - z)$, $(y + z)$, &c., are complete nth powers, or that they are of the form r^n, s^n, t^n, &c.: which proposition I first demonstrated in the 2d edition of the English translation of Euler's Algebra.

Now from what we have before observed, x, y, and z, may be considered as prime to each other, and since $x > y$, make $x = y + r$, then since x is prime to y, r must also be prime both to x and y; for if y and r had a common measure, x must have the same, because $x = y + r$, and if r and x had a common measure, y must have the same, since $x - r = y$; therefore, as x is prime to y, r is prime to both x and y. Now substituting for x our equation becomes Demonstration.

$$(y + r)^n - y^n = z^n,$$

from the developement of which, and substituting for the coefficient of $(y + r)^n$

1, n, $n\,a$, $n\,b$, &c., a, n, 1 (Cor. Lem. 3), we obtain

$n\,y$

$n\,y^{n-1}r + n\,a\,y^{n-2}r^2 + n\,b\,y^{n-3}r^3$, &c., $n\,y\,r^{n-1} + r^n = z^n$, or

$(n\,y^{n-1} + n\,a\,y^{n-2}r + n\,b\,y^{n-3}r^2$, &c., $n\,y\,r^{n-2} + r^{n-1})\,r = z^n$.

And here it is evident, that the first side of the equation is divisible by r, *once*, and after that, neither by r, nor by any factor of r, except n be one of its factors, in which case the first side is divisible by $n\,r$, *once*, and after that, neither by n, nor r, nor by any factor of r; because the first term y^{n-1} has no common measure with r, but all the rest of the terms have; and, therefore, the whole quantity, taken collectively, has no common measure with r. And the same must necessarily be true of the equal quantity x^n, namely, that it is divisible by r, *once*, and after that, neither by r, nor by any factor of r, unless n be one of its factors, in which case it is divisible by $n\,r$, *once*, and after that, neither by n, nor r, nor by any factor of r; therefore r, in the first case, and $n\,r$, in the second case, must be a complete nth power (Lemma 2): but if $r\,n = \varphi^n$, n must be a factor of φ, that is $\varphi = n\,\varphi'$, therefore, $r\,n = n^n\varphi'^n$, or $r = n^{n-1}\varphi'^n$, that is $r = x - y$ must be of one of the former, φ^n, or $n^{n-1}\varphi'^n$.

And it is evident that we should have been led to the same result, if we had considered the equation under the form

$$x^n - z^n = y^n,$$

namely, that $x - z$ must also be of one of the forms φ^n or $n^{n-1}\varphi^n$.

If we investigate the same equation under the form $y^n + z^n = x^n$, and make $y + z = s$, or $s - y = z$, we shall find that s is prime to both y and z, for if s and y had a common measure, z must have the same, and if s and z had a common measure, then y must have the same, because $s - z = y$; since, therefore, y and z are prime to each other, s is also prime to both. Substituting therefore for z as above, our equation becomes

$$(s - y)^n + y^n = x^n$$

Or by expanding the binomial $(s - y)^n$ and substituting for the coefficients as before, we obtain

$s^n - n\,s^{n-2}y + n\,a\,s^{n-3}y^2 - n\,b\,s^{n-4}y^3 + \&c.\ n\,s\,y^{n-2} = x^n$

or

$(s^{n-1} - n\,s^{n-2}y + n\,a\,s^{n-3}y^2 - n\,b\,s^{n-4}y^3 + \&c.\ n\,y^{n-2})\,s = x^n$

And here, from the same reasoning as that employed above, we find that s must be of one of the forms φ^n, or $n^{n-1}\varphi^n$.

Hence then if the equation

$$x^n - y^n = z^n$$

be possible, the following conditions must obtain, viz.

The difference of the roots $x-y$, of the form r^n or $n^{n-1}\,r^n$.
The difference of the roots $x-z$, of the form s^n or $n^{n-1}\,s^n$.
The sum of the roots $y+z$, of the form t^n or $n^{n-1}\,t^n$.

But since $(x-y)$ or $(x-z)$ and $(y+z)$ are respectively divisors of the three nth powers, z^n, y^n, and x^n, and since these three quantities are prime to each other, their divisors must also be prime to each other, and consequently only one of these can be of the latter form above given, as they would otherwise have a common divisor n. Therefore, if the equation be possible, we shall have either

$$\begin{cases} x-y \text{ of the form } r^n \\ x-z \ \ldots\ldots\ldots\ s^n \\ y+z \ \ldots\ldots\ldots\ t^n \end{cases}$$

or, two of these quantities will be of this form, and the third of the form $n^{n-1}\varphi^n$ which evidently resolves into the four following cases, one of which must necessarily obtain if the equation $x^n - y^n = z^n$ be possible, viz.

1st. $\begin{cases} x-y = r^n \\ x-z = s^n \\ y+z = t^n \end{cases}$ 2nd. $\begin{cases} x-y = n^{n-1}r^n \\ x-z = s^n \\ y+z = t^n \end{cases}$

3d. $\begin{cases} x-y = r^n \\ x-z = n^{n-1}s^n \\ y+z = t^n \end{cases}$ 4th. $\begin{cases} x-y = r^n \\ x-z = s^n \\ y+z = n^{n-1}t^n \end{cases}$

Q. E. D.

Proposition 2.

The equation $x^n - y^n = z^n$ is impossible in integers, n being any prime number greater than 2. Prop. 2.

We have before observed that x, y, and z, may be considered as prime to each other, and by the foregoing prop. it Demonstration.

is

is shown, that, if the equation be possible, one of the four following conditions must obtain, viz.

$$\text{1st}\begin{cases} x-y=r^n \\ x-z=s^n \\ y+z=t^n \end{cases} \qquad \text{2nd}\begin{cases} x-y=n^{n-1}r^n \\ x-z=s^n \\ y+z=t^n \end{cases}$$

$$\text{3d}\begin{cases} x-y=r^n \\ x-z=n^{n-1}s^n \\ y+z=t^n \end{cases} \qquad \text{4th}\begin{cases} x-y=r^n \\ x-z=s^n \\ y+z=n^{n-1}t^n \end{cases}$$

But at present we shall only consider one of those cases, for example the first, and in our result, by substituting $n^{n-1}r^n$ instead of r^n, $n^{n-1}s^n$ for s^n, &c., we shall arrive at all the possible cases. First then, let us ascertain whether the equation $x^n-y^n=z^n$ be possible, on the supposition that

$$\begin{aligned} x-y &= r^n \\ x-z &= s^n \\ y+z &= t^n \end{aligned}$$

Now from these three equations we derive the three following, viz.

$$\begin{aligned} x &= \tfrac{1}{2}\left(t^n+(s^n+r^n)\right) \\ y &= \tfrac{1}{2}\left(t^n+(s^n-r^n)\right) \\ z &= \tfrac{1}{2}\left(t^n-(s^n-r^n)\right) \end{aligned}$$

And consequently since $x^n-y^n=z^n$, or $x^n=y^n+z^n$, therefore

$$\left(\frac{t^n+(s^n+r^n)}{2}\right)^n=\left(\frac{t^n-(s^n-r^n)}{2}\right)+\left(\frac{t^n+(s^n-r^n)}{2}\right)^n$$

or

$$(t^n+s^n+r^n)^n=(t^n-s^n-r^n)+(t^n+s^n-r^n)^n$$

now

$$(t^n+\overline{s^n-r^n})^n=t^{nn}+nt^{nn-n}(s^n-r^n)+n\,a\,t^{nn-2n}.(s^n-r^n)^2+\&c.$$

$$(t^n-\overline{s^n-r^n})^n=t^{nn}-nt^{nn-n}(s^n-r^n)+n\,a\,t^{nn-2n}(s^n-r^n)^2-\&c.$$

$$(t^n+\overline{s^n+r^n})^n=t^{nn}+nt^{nn-n}(s^n+r^n)+n\,a\,t^{nn-2n}(s^n+r^n)^2+\&c.$$

And here, since the sum of the two first expressions is equal to the third, it is evident that the latter subtracted from the sum of the two former is equal to zero. But in adding the two first together, the 2nd, 4th, &c. terms cancel, and consequently in subtracting the latter from that sum, the

the 2d, 4th, &c. terms will remain the same, except that the signs will be changed from + to —. And as to the 1st, 3d, &c. terms of the first two equations, and the same terms of the third, we shall have (by observing that

$(s^n - r^n)^2 = (s^n + r^n)^2 - 4 s^n r^n$
$(s^n - r^n)^4 = (s^n + r^n)^4 - 8 (s^{3n} r^n + s^n r^{3n})$
$(s^n - r^n)^6 = (s^n + r^n)^6 - 12 s^{4n} r^n - 40 s^{3n} r^{3n} - 12 s^n r^{5n}$
&c. &c.) for the sum of the two

1st terms $2 t^{nn}$
3d terms $2 n a t^{nn-2n} (s^n + r^n)^2 - 2 n a t^{nn-2n} \times 4 s^n r^n$
5th terms $2 n c t^{nn-4n} (s^n + r^n)^4 - 2 n c t^{nn-4n} \times 8 s^n r^n (s^{2n} + r^{2n})$
7th terms, &c.

And, consequently, subtracting from those sums, the 1st, 3d, &c. terms of the third line, namely,

1st term t^{nn}
3d term $n a t^{nn-2n} (s^n + r^n)^2$
5th term $n c t^{nn-4n} (s^n + r^n)^4$

the remainders of these particular terms will be

1st rem. $= t^{nn}$
3d rem. $= n a t^{nn-2n} (s^n + r^n)^2 - 2 n a t^{nn-2n} \times 4 s^n r^n$
5th rem. $= n c t^{nn-4n} (s^n + r^n)^4 - 2 n c t^{nn-4n} \times 8 s^n r^n (s^{2n} + r^{2n})$
7th rem. &c. &c.

In short, the whole of the remainder which is equal to zero, will be expressed by

$$(t^n - \overline{s^n + r^n})^n - (2 n a t^{nn-2n} \times 4 s^n r^n) - (2 n c t^{nn-4n} \times 8 s^n r^n)(s^{2n} + r^{2n}) - \&c.$$

And here it is only neccessary to observe, that all the terms on the latter side of this expression are divisible by $t^n s^n r^n$, so that, for perspicuity sake, we may write it thus

$$(t^n - \overline{s^n + r^n})^n - t^n s^n r^n A = 0$$

and consequently

$$(t^n - \overline{s^n + r^n})^n = t^n s^n r^n A$$

and here, since the first side is a complete nth power, the latter side, which is equal to it, muſt be so likewise, and consequently

quently A must be a complete nth power, or $A = A'^n$; that is

$$(t^n - s^n + r^n)^n = s^n s^n r^n A'^n$$

and, therefore,

$$t^n - s^n - r^n = t s r A',$$

or, dividing by $t r s$, we have

$$\frac{t^{n-1}}{sr} - \frac{s^{n-1}}{tr} - \frac{r^{n-1}}{st} = A',$$

which must necessarily be an integer. But these three fractions are each in their lowest terms, because r, s, and t, are prime to each other, and each of the denominators contains a factor that is not common to the other two; they cannot therefore be equal to an integer, by cor. Lemma 1, and consequently the equation is impossible under the first condition. And in order to arrive at the results of the other three conditions, we have only to substitute $u^{n-1} r^n$ instead of r^n, $v^{n-1} s^n$ for s^n, and $u^{n-1} t^n$ for t^n, whence we draw the following four conclusions,

$$\text{1st} \quad \frac{t^{n-1}}{rs} - \frac{s^{n-1}}{tr} - \frac{r^{n-1}}{st} = A'$$

$$\text{2nd} \quad \frac{t^{n-1}}{rs} - \frac{s^{n-1}}{tr} - \frac{u^{n-1} r^{n-1}}{st} = A''$$

$$\text{3d} \quad \frac{t^{n-1}}{rs} - \frac{u^{n-1} s^{n-1}}{tr} - \frac{r^{n-1}}{st} = A'''$$

$$\text{4th} \quad \frac{u^{n-1} t^{n-1}}{rs} - \frac{s^{n-1}}{tr} - \frac{r^{n-1}}{st} = A''''$$

according as we assume the 1st, 2nd, 3d, or 4th, condition. And in each of these expressions we ought to have A', A'', A''', A'''', integer numbers, if the given equation were possible; but since in each of these expressions we have three fractions each in its lowest terms, and the denominator of each contains a factor not common to the other two, therefore by Lemma 1, and its corollary, they cannot produce an integer number.

Having shown, therefore, that, if the equation $x^n - y^n = z^n$ were possible, one of the quantities A', A'', A''', or A'''', would

would be an integer, and having also demonstrated, that no one of these quantities can be an integer, it follows that the equation whence they were derived is impossible; that is the equation $x^n - y^n = z^n$ is impossible, when n is a prime number.

But we have also shown, that the impossibility of the equation $x^n - y^n = z^n$, when n is a prime, involves with it the impossibility of every equation of the form

$$x^n + y^n = z^n,$$

in which n is any number whatever except 2, or some power of 2; and we have likewise shown, that the impossibility of the equation, when n is any power of 2, is involved in that of $x^4 - y^4 = z^4$, which particular case has been proved to be impossible by Euler; and consequently the equation

$$x^n + y^n = z^n$$

is always impossible, when n is any integer number whatever greater than 2. And since the equation $x^n \pm y^n = z^n$, is impossible, so also is $\frac{x^n}{m^n} \pm \frac{y^n}{p^n} = \frac{z^n}{q^n}$, for this is the same as $x^n + y^n = \frac{z^n m^n p^n}{q^n}$: and therefore the equation is likewise impossible in fractions.

Q.E.D.

V.

Method of raising large Stones out of the Earth; by Mr. ROBERT RICHARDSON, *of Keswick, in Cumberland**.

GENTLEMEN,

I, Robert Richardson, of Keswick, in the parish of Crosthwaite, and county of Cumberland, beg leave to inform you, that I have found out a method of taking large self-stones out of the ground in a very expeditious manner; and that by this means two men will take as many stones out of the ground in one day, as would require twelve men in

New method of taking large detached stones out of the ground.

* Trans. of the Soc. of Arts, vol. xxvi, p. 190. The silver medal was voted to Mr. Richardson for this invention.

the

the usual way of blasting, and afterward using large levers, &c.

Where stones from two to four tons each are to be taken up, two men will raise as many as twenty men in the usual way. The work is done by the power of a tackle, but by my method of fixing the tackle to the top of the stone, by the plug which I have invented, it will hold till the stone is pulled out of the ground, and laid upon the surface, or upon a carriage, if required, all which can be done in a very little time.

Stones of four tons weight, or upwards, may be taken out of the ground within the time of five or ten minutes, by two men, without any earth or soil being previously taken from around them, or without any digging with hacks or spades. J. C. Curwen, Esq., of Workington, has seen and approved of my performance with this invention, and if the Society should think it deserving of a premium, it would ever be gratefully acknowledged by

Gentlemen,

Your most obedient humble servant,

ROBERT RICHARDSON.

Keswick, Feb. 8, 1808.

DEAR SIR,

Efficacy of this method. I cannot suffer Mr. Richardson's letter to be sent to the Society, without adding a few lines concerning it. I can bear ample testimony to the ease, with which the largest self-stones are lifted by this method. I have seen one upwards of five tons lifted by four men. One of the plugs is sent for the inspection of the Society. There is no difficulty in cutting the hole to receive it, the only care is not to make it too large. It is difficult to explain the theory of its action; the least stroke laterally disengages the stone.

Its uses. In many situations it is likely to be of great use, not only in drawing stones out of the ground, but in making weirs and embankments, where the stones are only to be lifted a moderate height.

One of my farmers in Westmoreland has made great use of one, and speaks of it in high terms. I have exhibited it to numbers of persons, who could not believe its power, till they saw it tried.

Mr.

Mr. Richardson submits its examination to the Society, and I conceive it will be very useful and beneficial in cases of new enclosures of land. I do not think it would answer for soft stones, or safe to use for raising stones in buildings, it being so easily disengaged by any lateral blow. By adding wheels to the tackle machine, or having it upon a sledge, a great deal of time and trouble would be avoided. I purpose to employ this method next summer in making an embankment against the sea; the facility it will give in raising and removing large stones will expedite the work greatly. If any farther certificates of the performance of this plug be required by the Society, I will with pleasure transmit them to you. I will answer for its extracting any stone not exceeding five tons weight out of the ground, without any previous moving of the earth; and it is of importance to preserve large stones entire.

I am, with respect, dear Sir,
Your obedient humble Servant,
J. C. CURWEN.

Workington Hall, Feb. 19, 1808.

Sir,

I am favoured with your letter, desiring my opinion of the utility of the iron plug invented by Robert Richardson, of Keswick. That which I use is about six inches long, and one inch and a quarter in diameter; it requires a hole of its own size, only two inches deep; the plug is to be driven in a little short of the bottom, and will raise a stone of six or eight tons, with the assistance of three men, in the course of ten minutes after the hole is prepared; and I do not hesitate to say, that three men, thus furnished, will clear the ground of large stones in less time, and more effectually, than twelve men by any other method yet come to my knowledge. The plug should be made of good beaten iron. The simplicity and cheapness of the whole apparatus is a great object, as a good plug of the size I use will cost only two shillings and sixpence. I am fully of opinion, that by adding more and stronger ropes and pullies, work might be done by it to an amazing extent. I have reaped great advantage in my farm from the aid of the iron plug, Further remarks on it.

plug, and, in justice to the inventor, am happy in thus vouching for its extreme usefulness. Several of my respectable neighbours have experienced the aid and benefit of the above instrument, and will vouch, if required, for the truth of the above statement.

I am, Sir,

Your truly obedient Servant,

ROBERT WRIGHT.

Rose Gill Hall, near Shap, Westmoreland, May 9, 1808.

Reference to the Engraving of Mr. Richardson's Invention for raising large Stones out of the Earth. See Pl. VI, Figs. 4, 5, 6, *and* 7.

The method described.

Fig. 4, K, shows the upper part of a stone nearly buried in the earth, having a hole made in it three inches and a half deep, and one inch in diameter, by means of a miner's jumper; the cylindrical tail of the plug *a*, figs. 5, 6, and 7, which is of the same size, is driven fast into it, by means of a hammer applied upon the head of the plug at G. This plug, in its whole length, is nine inches, and has a hole made in its broad part H, through which the oval iron ring B passes easily, and on which the plug can move backwards and forwards, when the ring is hung upon the hook of the lower pulley block of the lifting tackle. C C C C represent the four legs or frame work of the quadrangle; D a five-fold tackle, with blocks ten inches in diameter; E a roller seven inches in diameter, turned by two long iron levers *b b*; the handle I is used as a safeguard, and to assist to regulate the power of the levers. In fig. 4, the plug A is shown fixed in the stone K, ready to draw it out of the ground, by means of the lifting tackle.

N.B. The hinder legs of the quadrangle are made to close in between the fore legs, for the convenience of carriage.

VI.

An Account of a new Method of increasing the charging Capacity of coated Electrical Jars, discovered by JOHN WINGFIELD, *Esq., of Shrewsbury*, and communicated to Mr.* JOHN CUTHBERTSON, *Philosophical Instrument Maker, of Poland Street, Soho; with Experiments proving the above, by Mr.* JOHN CUTHBERTSON.

IN my treatise, entitled *Practical Electricity and Galvanism*, p. 103, I have said, that breathing into coated jars increased their charging capacity to such an astonishing degree, that their discharge would fuse four times the length of wire they could do in ordinary circumstances, which I proved by experiment, p. 178, 194. Breathing into jars increases their capacity for a charge.

Since that publication large electrical batteries are become more general, and the number of jars increased, so that batteries containing thirty, sixty, and even a hundred and more jars are frequently met with. When so numerous, breathing into each jar is very disagreeable; and not only that, but, when the atmosphere is very dry, and when it is most wanted, it is even ineffectual; because the jars, which were first breathed into, lose that property which was produced in them by breathing, before the last can have obtained it; so that a variety of other means have been tried, such as wetting their insides with water, and putting wet sponges into them, and also greasing and oiling the uncoated part in the inside, all of which gave unsatisfactory results; till John Wingfield, Esq., communicated to me, that pasting paper on the inside and outside of coated jars prevented them from exploding to the outside coating, and that he believed their charging capacity was increased thereby. This inconvenient in large batteries, and moistening with water ineffectual. Paper pasted inside and out.

I embraced the first opportunity to try the effect of this discovery with single jars. Trials of this.

Exp. 1. I took a very thick jar (which had been used Exp. 1.

* A gentleman who has lately very much distinguished himself, not only in the electrical science, but in all other branches of experimental philosophy.

to show the phenomena of voluntary explosions without breaking) twelve inches high, and the coating nine inches, containing in the whole 171 square inches. It was applied to the conductor of a plate electrical machine, and six turns of the plate caused a voluntary explosion. The state of the atmosphere not being very dry, it required eight and twelve turns, to produce a second and third explosion. A fourth could not be produced; but, when cleaned and dried, as before, six turns caused a voluntary discharge.

Exp. 2. *Exp.* 2. A slip of paper one inch broad was taken of sufficient length to fit the outside of the jar, when the two ends were pasted together. This was slipped on the outside to about one inch from the coating, the uncoated part being rubbed clean and dry, and applied to the machine, eleven turns of the plate produced a voluntary discharge to the ontside coating.

Exp. 3. *Exp.* 3. The paper ring was then slipped down to touch the coating, and then applied to the conductor, no voluntary discharge could be produced, and when discharged in the common way, its power did not seem to be increased.

Exp. 4. *Exp.* 4. The common discharging electrometer (which is always fixed to the basement of my machine) was used, to try to what distance the discharge could be made to pass from the knob of the conductor to the ball of the electrometer, which was found to be one inch and $\frac{1}{4}$.

Exp. 5. *Exp.* 5. A piece of iron wire $\frac{1}{100}$ part of an inch in diameter, and one inch in length, was hung to the electrometer, through which a second discharge was made to pass, and the colour of the wire was changed to a blue.

Exp. 6. *Exp.* 6. The paper ring was then taken off, and I breathed into the jar twice; the discharge was then produced at the distance of two inches, and the wire was fused into balls.

Exp. 7. *Exp.* 7. The jar was then rubbed clean and dry, and a piece of the same sort of wire, of the same length, was hung to the electrometer in the same manner as before, and it appeared, that the greatest charge it could take had not the least effect upon the wire. Thus it appears, that a paper ring so applied does not increase the charging capacity of jars in the same degree as breathing.

Exp.

Exp. 8. The jar was highly charged, and examined in the dark. The paper ring appeared luminous all round the uppermost edge. Exp. 8.

Exp. 9. The ring was then taken off, and pasted on in the inside close to the coating. Twenty-three turns caused a voluntary explosion, through the ring, to the outside coating. Exp 9.

Exp. 10. A second ring, $\frac{1}{4}$ of an inch broad, was then pasted on close to the other. The same number of turns produced a voluntary explosion, and the paper was torn by the discharge; after which it was repaired, and left to dry. Exp. 10.

Exp. 11. When dry, no voluntary explosion could be obtained. Exp. 11.

Exp. 12. Its greatest power was then tried, and it was found exactly the same as in Exp. 6, when it was breathed into; it discharged at 2 inches distance, and the same length of wire was fused into balls. Exp. 12.

Exp. 13. A second jar was taken of a larger size, being 13 inches high, and its coating 7 inches, it contained about 190 square inches. After being rubbed clean and dry it was applied to the conductor of the machine, 12 turns of the plate produced a voluntary explosion to the outside coating. Exp. 13.

Exp. 14. A paper ring was put round the uncoated part on the outside, at about $1\frac{1}{2}$ inch distant from the coating. Eleven turns of the plate produced a voluntary explosion to the outside coating. The paper ring was then pushed down to the coating, after which no voluntary explosion to the coating could be obtained, but it discharged to the electrometer ball standing at the distance of 2 inches and $\frac{1}{2}$ from the knob of the conductor. Exp. 14.

Exp. 15. The same sort of wire as used in Exp. 6, two inches long, was hung to the electrometer, and the discharge made it blue, with several bendings: a proof that it had been nearly redhot. Exp. 15.

Exp. 16. A ring of common writing paper, one inch broad, was pasted on the inside close to the coating; and when dry no voluntary explosion to the coating could be obtained, but it discharged itself to the electrometer ball Exp. 16.

standing at the distance of 2¼, and the wire was fused into balls.

Exp. 17. *Exp.* 17. The paper rings were now taken off, and the uncoated part made clean and dry. Nineteen turns produced a discharge to the electrometer ball at the same distance, and the same length of wire was slightly blued.

Exp. 18. *Exp.* 18. The jar was then breathed into, and a discharge was produced at the same distance, but the wire was not fused.

Exp. 19. *Exp.* 19. The same jar was breathed into a second time; a discharge was caused at the same distance, and the wire was fused into balls exactly the same as when the paper rings were on.

Exp. 20. *Exp.* 20. A third jar was taken, 7 inches high and 4 inches diameter, having about 64 square inches coating. When rubbed clean and dry, and applied to the machine, two turns of the plate caused a voluntary discharge to the outside coating.

Exp. 21. *Exp.* 21. A paper ring was pasted on both sides, close to the coating, and one inch from the top. When applied to the machine, no voluntary explosion could be obtained, but the electric fluid was seen to run over the brim of the glass to the coating as quickly as the machine could give it. The exploding distance to the electrometer was seven eighths of an inch.

Exp. 22. *Exp.* 22. The paper rings were taken off, and others pasted on, to within one inch and a half from the top. No voluntary explosion to the coating could be obtained; but it exploded to the electrometer standing at one inch distance, and did not produce any effect on one inch of wire, which was hung to the electrometer.

Exp. 23. *Exp.* 23. Then paper rings of different breadths were tried, and it was found, that, when they were only half an inch higher than the coating, the jar received the greatest power, and its exploding distance to the electrometer was one inch and three eighths, which fused and dispersed in balls one inch of wire, of the same diameter as that used in Exp. 6, with the first jar.

Exp. 24. *Exp.* 24. The paper ring was scraped from off both sides, and the jar was carefully breathed into. Then six turns of the

the plate caused its longest discharge to the electrometer, which was at the distance of one inch and three eighths, and fused one inch of wire, but with less violence than in the last experiment.

Exp. 25. The outside paper ring was scraped off, and the jar still preserved the same charging capacity, as when both were on. Exp. 25.

The above experiments are sufficient to prove, that paper rings pasted on to electrical jars not only prevent them from exploding to the outside coating before they have received their highest charge, but that they likewise increase their charging capacity; and that one ring pasted on in the inside only is sufficient, if it is one inch broad; one half of the breadth must be pasted upon the coating, and the other upon the uncoated part. General conclusions.

Farther experiments and observations, setting forth the advantages that electricians may obtain from the above discovery, with an account of some experiments done with a view to prevent the jars from being perforated by high charges, without increasing their thickness, wherein I am in hopes I shall succeed, will be the subject of a future paper.

VII.

On the Combinations of Oxigen. By MARSHALL HALL, *Esq. F. R. M. S. E. In a Letter from the Author.*

To Mr. NICHOLSON,

SIR,

THE utility and excellence of axioms in science are too well known to those, who are earnestly engaged in its prosecution, to require to be expatiated on. The few observations on the combinations of oxigen, which I take the liberty of transmitting to you, do not perhaps deserve the dignified name of axioms; but where coincidences are so general and striking, we are led, perhaps too soon indeed, to believe them universal. If however I shall point out what generally takes Axioms of great use in science.

takes place, the subject will not want importance and interest to those who are about to begin the elevated study of chemistry: to such it will at least afford a useful aid to the memory, much to be desired.

The first two of these propositions will be perceived to be very generally known: it is the third, which is the most extraordinary, and which renders the former of more importance.

Laws of the combinations of oxigen.

First then it is known, that substances containing oxigen will unite to *each other:* and 2dly, substances which do not contain this principle readily and *mutually* combine: but, 3dly, I believe no substance containing oxigen will combine with a substance, which does not contain oxigen.

Acids combine with oxides, but not with metals.

I shall now endeavour to elucidate and support the preceding propositions; and no circumstance can do this more forcibly, than the universal chemical fact, that metals do not dissolve in *acids*, until they have, by some means, acquired oxigen; but the instant the oxidation is accomplished, the solution takes place. In the same manner the alkalis do not combine with the metals, but the metallic oxides and the alkalis do combine, *e. g.* the ammoniuret of copper, &c.; again, the metals during the process of oxidation immediately *separate* from the oxide when formed. But to reduce the subject into its simplest form, we may observe—

Alkalis do the same.

Metals separate from oxides as soon as formed.

Classification of bodies that combine.

1. That the metals, metalloids, and simple combustible bodies, combine with each other. 2. The oxides of the metals, the alkalis, and the acids, formed of the combustible bodies with oxigen, mutually combine. 3. But no part of the first class will combine with any part of the second. 4. The first class have no affinity for water. 5. The second class, and most of the saline substances, do dissolve in water.

Objections answered.

It will now be incumbent on me to obviate some objections, which may naturally oppose what I have said; and the most prominent is the union of the combustible bodies with the alkalis and metallic oxides. But this objection is readily answered; for those combustibles, which enter into such a combination, really contain oxigen; they are sulphur, phosphorus, and sulphuretted hidrogen. I think it sufficient

Union of alkalis and metallic oxides with combustibles;

cient to observe, that from the late experiments of Mr. Davy, the former two are found to be oxides; and hence their combinations with the alkalis do not at all invalidate the preceding doctrine, but tend to confirm it. The sulphuretted hidrogen too does most probably contain oxigen, as every body has suspected, from its properties as an acid, even that distinctive one of reddening vegetable blues. "Kirwan was unable to form it by melting sulphur in a "vessel containing hidrogen gas; and the Dutch chemists "were equally unsuccessful either with this method, or by "passing hidrogen gas through a tube containing liquid "sulphur*." From this then it appears, that the presence of atmospheric air is necessary to form the sulphuretted hidrogen, and hence we may infer with probability, that oxigen enters into the combination.

If sulphur contain oxigen, then it will naturally be asked, why does it combine with the metals, which do not contain oxigen? To this I am ready to answer, that it is in some cases certain, in others more than probable, that the metal is oxidated during the operation; not indeed from the atmosphere, or other external sources, but from the sulphur itself; and this need not appear strange, for in the same manner the metals are oxidated by the acids, which afterward dissolve them: but this is not hypothesis, it is founded on the most unexceptionable experiments. Berthollet "formed "metallic sulphurets, performing the experiments in an "earthen retort, and after taking every precaution to avoid "any source of uncertainty, he obtained sulphuretted *hi*-"*drogen*; the metals he used were iron, copper, and mer-"cury, the last afforded the largest quantity†." Here then is unequivocally the decomposition of the sulphur; and the experiment is a more important argument, because it was made with a very different view from that with which I have applied it, *i. e.* to prove, beyond the possibility of mistake, that, during the formation of metallic sulphurets, hidrogen is liberated; and, as this is proved, it remains, that the combination in the retort will contain a larger proportion of the other principles of the sulphur, one of which is oxigen. and of sulphur with metals.

* Murray. † Murray.

And

Production of heat and light in the formation of sulphurets.

And here I cannot help observing, that a very beautiful application may be made of this circumstance, to explain the production of heat and light during the formation of these sulphurets, a difficulty hitherto inexplicable. It is well known, that oxigen in some of its combinations retains more of light and heat than in others, now here is the transition of oxigen, as contained in sulphur, to a metal; it is by no means improbable, that in the latter compound it may be disposed to retain less of the light and heat than in the former; if this be the case, the appearances during the operation no longer present any difficulty.

Union of alkalis with oil.

The only other circumstance, which occurs in objection to the above propositions, is the combination of alkali and oil; and this objection also is very readily satisfied. "When "the oil is separated by an acid from soap, it is affirmed, "that in several respects its properties are altered, and that "in particular it is soluble in alcohol. During its con-"version into soap, therefore, it must not merely have "combined with the alkali, but have undergone some other "change*." But a short quotation from La Grange is still more to the purpose. "There is," he says, "an absorp-"tion of oxigen during the process of saponification; that "is to say, the oil becomes concrete by absorbing oxigen." There is indeed still another compound, which may be urged as unfavourable to this doctrine, viz. the solution of sulphur and phosphorus in oil, but it is equally invalid with the former, and another quotation from Mr. Murray's excellent work will again calm the clamour of objection. It has been remarked, that sulphur and phosphorus contain oxigen; now this is the composition of oil in the words of Mr. Murray. Lavoisier hence inferred, "that oil contains "79 parts of carbon united to 21 of hidrogen. These must "however be regarded as approximations only; and *oxigen* "is probably also a constituent part of oil. That it is so "appears to be *established* by the decomposition of oil in "close vessels; when transmitted through an ignited tube, "carbonic acid and water are part of the products."

Solution of sulphur and phosphorus with oil.

It is however evident, that these observations are intended to apply in cases more simple than to vegetable compounds,

* Murray.

in

in which indeed the elementary ingredients generally acquire a perfectly different arrangement, in every case of combination with other substances.

I shall just add a remark, which I have often made, that substances not containing oxigen are perfectly *mild* in their nature, and only acquire acrid or virulent properties when oxigen is added to them. Thus oxigen occasions the acidity, the alkaline property, and, as has been remarked to me, the causticity of other substances. To this ingredient all mineral remedies owe their powers, to this the metallic and some other poisons owe their deleterious properties. Oxigen essential to acrimony.

If the above observations deserve to occupy a page or two in the valuable Philosophical Journal, their insertion will much oblige, Sir,

Your obedient servant,

MARSHALL HALL,
Member of the Royal Medical Society.

University of Edinburgh,
Sept. 21, 1810.

VIII.

On the Migration of Swallows. *By* THOMAS FORSTER, *Esq.* *In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

IN a former number of your Journal you were so good as to insert my table of the times of appearance and departure of several of our migratory birds; if you should think the ensuing observations on the swallow tribe worthy of notice and insertion, they are very much at your service. Remarks on the migration of swallows.

The swift, *hirundo apus*, which abounds in the neighbourhood of Hackney, and annually builds in great numbers in the old steeple, was not seen at Hackney after August the 13th; and several days previous to this its numbers were greatly decreased. On the 17th I happened to be Swift.

be at Ely, when I saw great numbers of these birds flying round the tower of the cathedral; since which I have not seen a single bird.

Holes of the bank martin occupied by toads in winter.

In consequence of the controversy so long carried on among naturalists, whether the swallow was a bird of passage, or whether it remained dormant during winter, I opened on the 12th instant several of the holes of *sand swallows (hirundines ripariæ)*, but found nothing in them except *toads* which had taken up their winter's lodging there.

The migration of swallows depends more on the wind, than on the weather.

There are many circumstances, which tend greatly to establish the opinion, that these birds migrate; for instance, they do not appear in spring and depart in autumn sooner or later according to the forwardness or backwardness of the season, but generally according to the direction of the prevailing current of air. The great prevalence of N. E. winds lately has occasioned a very great diminution of the numbers of swallows and martins this year much before their usual time; whence may we not infer, that they had taken their flight in a south western direction? for winged insects (the food of this tribe) are still very abundant, notwithstanding the cold winds.

Inquiry after facts respecting their torpidity.

If any of your numerous readers know of any well authenticated accounts of swallows having been found in a torpid state during the winter season, and will have the goodness to communicate the same through the medium of your Journal, I shall be much obliged to them, as I am collecting facts of this kind.

I remain, Sir,

Your constant reader,

THOMAS FORSTER.

London, Oct. 15, 1810.

IX.

IX.

III. *The Case of a Man, who died in consequence of the Bite of a Rattlesnake; with an Account of the Effects produced by the Poison.* By EVERARD HOME, *Esq. F. R. S.**

OPPORTUNITIES of tracing the symptoms produced by the bite of poisonous snakes, and ascertaining the local effects on the human body when the bite proves fatal, are of such rare occurrence, that no well described case of this kind is to be met with in any of the records that I have examined. I am therefore induced to lay before this Society the following account, with the view of elucidating this subject, in which the interests of humanity are so deeply concerned.

Well described cases of fatal bites of snakes very rare.

Thomas Soper, 26 years of age, of a spare habit, on the 17th of October 1809, went into the room in which two healthy rattlesnakes, brought from America in the preceding summer, were exhibited. He teized one of them with the end of a foot rule, but could not induce the snake to bite it, and on the rule dropping out of his hand, he opened the door of the cage to take it out; the snake immediately darted at the hand, and bit it twice in succession, making two wounds on the back part of the first phalanx of the thumb, and two on the side of the second joint of the fore finger. The snake is between 4 and 5 feet long, and when much irritated bites the object twice, which I believe snakes do not usually do.

Man bitten by a rattlesnake.

The snake bit twice.

The bite took place at half past two o'clock. He went immediately to Mr. Hanbury, a chemist in the neighbourhood. There was at that time no swelling on the hand, and the man was so incoherent in his language and behaviour, that Mr. Hanbury considered him to be in a state of intoxication, and gave him a dose of jalap to take off the effects of the liquor, and made some slight application to the bites. It appeared on inquiry, that the man had been drinking; but that, before he was bitten, there was nothing

Effects of the bite.

* Abridged from the Phil. Trans. for 1810, p. 75.

unusual

Effects of the bite.

unusual in his behaviour. After leaving Mr. Hanbury the hand began to swell, which alarmed him, and he went to St. George's hospital. He arrived there at three o'clock. The wristband of his shirt had been unloosed, and the swelling had extended half way up the fore-arm before his admission. The skin on the back of his hand was very tense, and the part very painful. At four o'clock the swelling extended to the elbow, and at half past four it had reached half way up the arm, and the pain had extended to the axilla. At this time Mr. Brodie, who visited him in my absence, first saw him; he found the skin cold; the man's answers were incoherent; his pulse beat 100 strokes in a minute, and he complained of sickness.

At 9 o'clock he had the feeling of great depression, his skin was cold, and his pulse weak, beating 80 strokes in a minute. At a quarter after 10 the pain had become very violent in the arm: his pulse was stronger, but fits of faintness attacked him every fifteen minutes, in which the pulse was not perceptible; yet in the interval his spirits were less depressed. At half after 11 the hand and arm were much swelled, up to the top of the shoulder, and into the armpit. The arm was quite cold, and no pulse could be felt, even in the armpit, where the swelling was such as to prevent the artery from being accurately distinguished. The wounds made on the thumb were just perceptible; those on the finger were very distinct. His skin in general was unusually cold.

In the morning of the 18th his pulse beat 132 strokes in a minute, and was very feeble. The swelling had not extended upward to the neck, but there was a fulness down the side, and blood was extravasated under the skin as low as the loins, giving the back on the right side a mottled appearance. The whole of the arm and hand was cold, but painful when pressed; the skin was very tense; on the inside of the arm vesications had formed below the armpit and near the elbow, and under each of the blisters was a red spot of the size of a crown. The skin generally over the body had become warm. At noon the skin of the whole arm had a livid appearance, similar to what is met with in a dead body, when putrefaction has begun to take place, unlike

unlike any thing I had ever seen in so large a portion of the living body. An obscure fluctuation was felt under the skin of the outside of the wrist and forearm, which induced me to make a puncture with a lancet, but only a small portion of a serous fluid was discharged. Effects of the bite.

On the 19th his pulse was scarcely perceptible: his extremities were cold: the vesications were larger, the size of the arm was diminished, and he had sensation in it down to his fingers.

On the 21st the size of the arm was farther reduced, but the skin was extremely tender.

On the 22d the right side of the back, down to the loins, was inflamed and painful; and had a very mottled appearance from the extravasated blood under the skin.

On the 23d the vesications had burst. On the 26th the arm was more swelled and inflamed. The inflammation increased; and on the 28th a slough had begun to separate from the inside of the arm below the armpit. On the 29th a large abscess had formed on the outside of the elbow, which was opened, and half a pint of reddish brown matter was discharged with sloughs of cellular membrane floating in it. The lower part of the arm became much smaller, but the upper part continued tense.

On the 30th the redness and swelling of the upper part of the arm had subsided. On the 31st the discharge from the abscess had diminished. On the 1st of November ulceration had taken place on the opening of the abscess, so that it was much increased in size. The next day this ulceration had spread to the extent of two or three inches; and mortification had come on in the skin nearer the armpit. On the 3d the mortification had spread considerably; and the fore finger, which had mortified, was removed at the second joint. And on the 4th of November he died at half after four o'clock in the afternoon.

Sixteen hours after death, the body was examined by Mr. Brodie and myself, in the presence of Mr. Maynard, the house surgeon, and several of the pupils of the hospital. Appearances on dissection.

With the exception of the right arm, which had been bitten,

Appearances on dissection.

bitten, the body had the natural appearance. The skin was clear and white; and the muscles contracted.

The wounds made by the fangs at the base of the thumb were healed, but the puncture made by the lancet at the back of the wrist was still open. That part of the back of the hand, which immediately surrounded the wounds made by the fangs, for the extent of $1\frac{1}{2}$ inch in every direction, as also the whole of the palm, was in a natural state, except that there was a small quantity of extravasated blood in the cellular membrane. The orifice of the abscess was enlarged, so as to form a sore on the outside of the arm, elbow, and forearm, near six inches in length. Around this, the skin was in a state of mortification, more than half way up the outside of the arm, and as far downwards, on the outside of the forearm. The skin still adhered to the biceps flexor muscle in the arm, and flexor muscles in the forearm, by a dark coloured cellular membrane. Every where else in the arm and forearm, from the axilla downward, the skin was separated from the muscles, and between these parts there was a dark coloured fluid, with an offensive smell, and sloughs of cellular membrane resembling wet tow, floating in it. The muscles had their natural appearance every where, except on the surface, which was next the abscess. Beyond the limits of the abscess, blood was extravasated in the cellular membrane; and this appearance was observable on the right side of the back as far as the loins, and on the right side of the chest over the serratus major anticus muscle.

In the thorax the lungs had their natural appearance. The exterior part of the loose fold of the pericardium, where it is exposed on elevating the sternum, was dry, resembling a dried bladder. The cavity of the pericardium contained half an ounce of serous fluid, which had a frothy appearance, from an admixture of bubbles of air. On cutting into the aorta, a small quantity of blood escaped, which had a similar appearance. The cavities of the heart contained coagulated blood.

In the abdomen, the cardiac portion of the stomach was moderately distended with fluid: the pyloric portion was much

much contracted; the internal membrane had its vessels very turgid with blood. The intestines and liver had a healthy appearance. The gall bladder was moderately full of healthy bile. The lacteals and the thoracic duct were empty; they had a natural appearance. Appearances on dissection.

In the cranium the vessels of the pia mater and brain were turgid with blood; the ventricles contained rather more water than is usual, and water was effused into the cells, connecting the pia mater and tunica arachnoides. It is to be observed, that these appearances in the brain and its membranes are very frequently found in cases of acute diseases, which terminate fatally.

Mr. Home then adds two cases, that were sent from India to the late Dr. Patrick Russell, which correspond in many of the circumstances with the preceding; and an experiment he made formerly in the island of St. Lucia on the effects of the poison of a snake on two rats. The first case is that of a boy, who was bitten by a snake, called *kamnlee* by the natives, in the lower part of the arm, at 8 o'clock in the evening. The blood flowed very freely for some time. He died next day at noon in great pain.

The second is that of a sepoy, 60 years of age, bitten on the back part of the hand by a *cobra di capello*. He recovered, though slowly.

The paper concludes with the following observations.

It appears from the facts, which have been stated, that the effects of the bite of a snake vary according to the intensity of the poison. Effects of the bites of snakes different.

When the poison is very active, the local irritation is so sudden and so violent, and its effects on the general system are so great, that death soon takes place. When the body is afterward inspected, the only alteration of structure met with is in the parts close to the bite, where the cellular membrane is completely destroyed, and the neighbouring muscles very considerably inflamed. When the poison is very active.

When the poison is less intense, the shock to the general system does not prove fatal. It brings on a slight degree of delirium, and the pain in the part bitten is very severe: in about half an hour, swelling takes place from an effusion of serum in the cellular membrane, which continues to increase When less so.

crease with greater or less rapidity for about twelve hours, extending during that period into the neighbourhood of the bite; the blood ceases to flow in the smaller vessels of the swoln parts; the skin over them becomes quite cold, the action of the heart is so weak, that the pulse is scarcely perceptible, and the stomach is so irritable, that nothing is retained in it. In about 60 hours these symptoms go off, inflammation and suppuration take place in the injured parts, and when the abscess formed is very great, it proves fatal. When the bite has been in the finger, that part has immediately mortified. When death has taken place under such circumstances, the absorbent vessels and their glands have undergone no change similar to the effect of morbid poisons, nor has any part lost its natural appearance, except those immediately connected with the abscess.

Patients who recover.

In those patients, who recover with difficulty from the bite, the symptoms produced by it go off more readily, and more completely, than those produced by a morbid poison, which has been received into the system.

Supposed efficacy of medicines.

The violent effects which the poison produces on the part bitten, and on the general system, and the shortness of their duration, where they do not terminate fatally, has frequently induced the belief, that the recovery depended on the medicines employed; and in the East Indies eau de luce is considered as a specific for the cure of the bite of the cobra di capello.

There does not appear to be any foundation for such an opinion; for when the poison is so intense, as to give a sufficient shock to the constitution, death immediately takes place; and where the poison produces a local injury of sufficient extent, the patient also dies, while all slighter cases recover.

The effect of the poison on the constitution is so immediate, and the irritability of the stomach is so great, that there is no opportunity of exhibiting medicines, till it has fairly taken place, and then there is little chance of beneficial effects being produced.

Treatment.

The only rational local treatment to prevent the secondary mischief is making ligatures above the tumefied part, to compress the cellular membrane, and set bounds to the swelling,

swelling, which only spreads in the loose parts under the skin; and scarifying freely the parts already swoln, that the effused serum may escape, and the matter be discharged, as soon as it is formed. Ligatures are employed in America, but with a different view, namely, to prevent the poison being absorbed into the system.

X.

Analyses of various Minerals, by Mr. Klaproth.

(Concluded from p. 155.)

Analyses of talc and mica.

Among the minerals which are most commonly known there are several, the analysis of which deserves to be repeated, for the purpose of correcting their classification. Though talc and mica may be distinguished from each other in strongly marked specimens, which serve as types of the two species, they have a great deal of similitude in their external characters. But as nature is far from having separated minerals by limits as well marked, as those we are obliged to employ in our systems, in order to facilitate a knowledge of them, there are found between mica on the one hand, which belongs to the argillaceous genus, and talc on the other which belongs to the magnesian, a great many minerals, occupying various places between the archetypes of the two species, and perplexing the mineralogist in his determination where to place them.

Talc and mica require to be discriminated.

Thus Mr. Haüy has classed among the talcs several minerals, which he is apprehensive will not ultimately be allowed to retain their place. "I confess," he says, "that among the minerals I have included under the name of talc, there are perhaps several, which chemical analysis will not suffer to remain there: but it appears to me at present premature, to make any change in this part of the system; particularly as I find we have no analysis of pure talc, that is not very old. This, therefore, should be reexamined, that we may

the name of talc was employed in contradistinction to that of mica; talc signifying a mica in large laminæ, and mica a talc in small scales. It was supposed to have been observed too, that talc was softer to the feel, and mica more harsh; but the point of separation, where talc ceases to be mica, and mica talc, still remained to be determined."

To fix the boundary between these two minerals with accuracy, a strict investigation of the component parts of mica was still requisite. Accordingly Mr. Klaproth took the common mica of Zinnwalde, in the mountains of Bohemia, for this purpose. It is found over a tin mine; is of a silvery white mingled with gray; and is crystallized in hexagonical laminæ, a little elongated, of an elastic flexibility, and usually arranged in the form of a rose.

Common mica from Bohemia.

While this elastic kind of flexibility may serve as one of the external characters to distinguish mica from talc, which has only an ordinary flexibility, the manner in which mica comports itself on exposure to heat is still more characteristic. When mica is heated redhot, its silvery white is changed to a deeper gray, but no diminution of weight is observed. Before the blowpipe on a piece of charcoal it melts readily into a shining, rounded bead, of a grayish black. Exposed to the heat of a porcelain furnace in a clay crucible, it fuses into a dark opake glass; and in a charcoal crucible into a semitransparent glass, covered with grains of iron.

Action of heat.

The results of its analysis were

Component parts.

Silex	47
Alumine	20
Oxide of iron	15·5
——— manganese	1·75
Potash	14·5
	98·75.

3. *Siberian mica in large laminæ.*

Muscovy glass.

The mica in large laminæ, or Muscovy glass, called in Russia *slinda*, differs so much from common mica in the largeness of its laminæ, and in its splitting perfectly straight,

straight, that a chemical analysis was requisite, to determine whether it be in reality a variety of mica. It is chiefly used in Russia and Siberia instead of glass for windows, on which account it is an article of trade. It is found in the remotest parts of Siberia, on the other side of the river Lena, and almost always near the rivers Vitim and Mama. It occurs in a coarse-grained granite, and in large masses of quartz, either in nodules of different sizes, or in thick laminæ lying in various directions. It is got out with the mallet and chisel. As the rock is very hard, it is wrought only to the depth of a fathom by Russian colonists, who form companies for this purpose, and go and reside in the neighbouring woods during the summer. After the mica is got out of the quarry, it is sorted according to the size and clearness of the plates, and then carried to market, chiefly at Irkutzk, whence it is sent to various places.

The pieces fresh taken from the quarry, and not split, have a smoky or brownish colour, are opake, and reflect objects like a mirror; but in thin plates the colour disappears, and the mica is transparent. Their price varies considerably, according to the size of the plates: some are 36 or 40 inches square; but in general they are only three or four inches, and such as are less than this are worth very little.

Before the blowpipe, on charcoal, Muscovy glass loses its transparency, and becomes of a silvery white, but does not melt like common mica. If large plates of mica be heated redhot in crucibles, or on charcoal, they acquire a striking appearance of thin laminæ of silver, and experience a loss of 1·25 per cent. This mica is infusible even by the heat of a porcelain furnace. In a charcoal crucible, in which several of these laminæ, rolled one upon another, had been placed, the outer ones were found of a gray colour, glazed, and fragile; the inner ones were as black as tinder, and flexible. In a clay crucible all were hardened, vitrified, fragile, and sonorous; and their colour was of a grayish white, the surface only being in part light brown. Action of heat.

The

The results of its analysis were

Component parts.

Silex	48
Alumine	34·25
Oxide of iron	4·5
Magnesia, mixed with a little oxide of manganese	0·5
Potash	8·75
Loss by calcination	1·25
	97·25.

4. *Black Siberian mica.*

Black Muscovy glass.

Another variety, found in similar situations with the preceding, is the black mica, or black Muscovy glass. This differs both from the preceding and the common mica, not only in appearance, but in the proportions of its component parts. The following is Karsten's description of it.

When in large masses it appears black, but in thin plates it is a deep olive green. Before the laminæ are separated, they exhibit metallic reflections of green, blue, and red, on being held under different angles to the light. It may be obtained in large plates, and these split into thinner, which, by their tendency to form rhombs, indicate a secondary juncture. The principal fracture is lamellar, with very shining laminæ of a greasy and semimetallic lustre. This mineral is very tender, extremely smooth, and perfectly transparent when the laminæ are very thin, though entire pieces are opake. The laminæ have a perfectly elastic flexibility.

This substance is employed scarcely for any thing but lining little boxes either of wood or pasteboard.

Action of heat.

Before the blowpipe on charcoal it does not appear to undergo fusion except at the edges of the laminæ. If larger plates be heated redhot in a crucible, they acquire a tombac brown colour with a metallic brilliancy. The leaves split, and appear friable. They lose one per cent.

The

The results of the analysis are

Silex	42·5	Component parts.
Alumine	11·5	
Magnesia	9	
Oxide of iron	22	
——— manganese	2	
Potash	10	
Loss by calcination	1	
	98.	

From what has been said we may conclude:

1. That pure talc contains magnesia, and no alumine, which is a decided characteristic of this stone. Difference between talc and mica.

2. That common mica contains alumine, and no magnesia.

3. That Muscovy glass differs from common mica by its refractoriness, its larger proportion of alumine, its smaller proportion of oxide of iron, and its containing a trace of magnesia.

4. That the black mica of Siberia deserves to be considered as a variety differing both from common mica and Muscovy glass by its proportions of alumine and magnesia, as well as by its larger proportion of oxide of iron.

5. That mica, and its different varieties, are to be reckoned among the richest of the potassiferous minerals.

XI.

Description of the Dichroit, a new Species of Mineral: by Mr. L. CORDIER, *Mine Engineer in Chief**.

THE mineral I am about to describe belongs to the class of earthy substances. Its proper place in a system appears to be next the emerald; and it would not be more remarkable than most species of the same class, if it were not Mineral possessing a new property.

* Abridged from the Journal des Mines, vol. XXV, p. 129.

endued

endued with a particular property, the knowledge of which may be interesting perhaps to those philosophers, who study the course of light through crystallized mediums.

Where found. This mineral was found at Cape de Gattes, in Spain. It was already known to the inhabitants of the country, and the lapidaries of Carthagena, when Mr. Launoi, a dealer in minerals, visited the place about twenty years ago, and brought away some specimens, which have been sold, part in France, and part in Germany. Most of these specimens being badly defined, they added to collections a rarity, of which science took no account.

Not yet analysed. Being at Cape de Gattes a few years ago, I was fortunate enough to meet with some pieces of the mineral in question, all the essential characters of which were sufficiently decided, and indicated a new species. I purposed to give a description of it, as soon as I had analysed it; but not yet having had an opportunity of doing this, I am induced to

Mentioned by some German writers. publish my mineralogical observations on it, particularly as some foreign mineralogists have been beforehand with me. Mr. Reuss, in the last volume of his treatise published in 1806, announces, that Werner has made a new species of the substance from Cape de Gattes, by the name of *yolith*; that he has ranged it next the cat's-eye, and divided it into three varieties, the vitreous, porphyritic, and common. Mr. Karsten, adopting Werner's opinion, in his Mineralogical Tables for 1808, has placed the yolith between the lazulite and andalousite of Delamétherie, and gives the following description of it.

Karsten's description. "This mineral is found of a deep lavender blue, in mass or disseminated; of a feeble lustre, verging from brilliant to shining; with an uneven fracture, the fragments of which are indeterminate, and with very acute edges; the separate pieces are indistinct, and large grained. It is hard, brittle, opake, and moderately heavy. It is found at Cape de Gattes in Spain, associated with lithomarge, quartz, and crystallized almandine."

It is difficult to find in this description the characters, that induced Werner and Karsten to make a particular species of the mineral in question, for it is equally applicable

to

to varieties of known substances, and particularly to the blue tourmaline.

Before I proceed to describe the dichroit, I ought to observe, that it is not mentioned in Delamétherie's Theory of the Earth, Haüy's Treatise on Mineralogy, Patrin's work, Brongniart's, the Abstract of Haüy's Method by Lucas, or any other work yet published in France.

Hitherto the dichroit has been found only in amorphous or crystallized grains, sometimes collected in small masses, not four inches in diameter. How found.

Its essential character is its being divisible parallel to the faces of a regular hexaedral prism, and capable of subdivision by longitudinal sections perpendicular to the lateral faces. Essential character.

Its specific gravity is 2·56. Physical characters.

It scratches glass strongly, quartz feebly; and is easily broken.

Its fracture is vitreous, tolerably shining, and frequently giving very evident indications of scales.

Its fragments are irregular with sharp edges.

Its powder feels very rough.

The lustre of the external surface commonly dull.

The translucid crystals exhibit a particular phenomenon, which may be called that of double colour by refraction.

Its primitive form is a regular hexaedral prism. Geometrical characters.

Its integrant particle is a triangular prism, the bases of which are scalene rectangles.

It is not acted upon by acids. Chemical characters.

Before the blowpipe it fuses into a very light greenish gray enamel. A similar result is obtained either with borax or carbonate of soda.

The dichroit is distinguishable, 1, from the emerald, because the specific gravity of the latter is greater in the proportion of 10 to 9; its integrant particle is an equilateral triangular prism; and it fuses more difficultly: 2, from the tourmaline, in not becoming electrical by heat, and in being less hard, and less heavy: 3, from the corundum, in the latter being infusible, and affecting a rhomboidal primitive form: 4, from the dipyre, or leucolite, because the latter fuses with ebullition, and its powder is more phosphorescent: Distinguishing characters.

rescent: 5, from the nepheline, or sommite, because pieces of the latter immersed in nitric acid become cloudy internally, and its specific gravity is less in the proportion of four to five: 6, from the haüyne, in the property of the latter to resolve into a jelly in acids.

Varieties. There are four varieties. 1. The primitive dichroit, which is a regular hexaedral prism.

2. The peridodecaedral. A rectangled prism, with twelve faces inclined to each other at angles of 150°.

3. Amorphous. In large irregular grains, exhibiting the rudiments of crystallization.

4. Granular. In irregular masses, formed of very large grains confusedly aggregated.

Transparency. With respect to transparency, it is sometimes translucid, sometimes opake.

Colour. All the crystals, or grains, viewed by reflected light, are of a violet colour, which is generally less bright in the longitudinal direction of the prisms.

Double by refraction. All the translucid crystals or grains, seen by refracted light, are both of a brownish yellow and an indigo blue. When viewed parallel to the axis of the prism, they constantly exhibit a very deep blue: but when viewed perpendicularly to this axis, they are of a very light brownish yellow. In the second case the transparency appears to be increased in the proportion of six to one.

Where found. The dichroit is found in two places at Cape de Gattes; namely, at Granatillo, near Nijar, where its situation was verified anew last year by Mr. Tondi; and at the foot of the mountains surrounding the bay of San Pedro. The preceding description is drawn up from specimens from the latter place. They are found there in a vast horizontal ledge of volcanic breccia. This breccia is composed of detritus of every kind, but more particularly of fragments and blocks of black or red scoriæ in perfect preservation, of black vitreous lava, and of lithoid lava, either basaltic or petrosiliceous. The dichroit is found chiefly in blocks of the latter kind. Sometimes it occurs in the form of scattered grains, sometimes of crystals grouped, and as it were imbedded in the lava. It is found also not only in the gray or whitish tufa, which serves as a base to the breccia, but

also

also in some of the fragments of foliaceous granite, which it includes. These fragments have evidently been exposed to the action of heat; and the primitive stratum, from which they have been detached, is very probably the original matrix of the dichroït. In fact they exhibit in their composition scales of black mica, and trapezoidal red garnets similar to those we see contained in the masses, and even in the interior of the crystals of this mineral, which indicates a contemporaneous formation. The petrosiliceous lava, that commonly serves as a gangue, is rather granular than compact. It is of the same nature as that of the Ponce Islands, or that of the Puy-de-Dôme, and of the cascade of Mont-d'Or in France, being composed of very fine grains of feldspar. The fire has left some very evident traces of its action on the crystals and masses of dichroït: most of the masses appear as if corroded in different places, both internally and externally, and in the cavities are seen portions of white scoriæ, either intact or decomposed. The crystals are almost all partly fused, cracked, and full of flaws. Their fragments frequently present surfaces rendered dull by an extremely thin whitish coating, that conceals the lustre of the fracture.

Reasons for making it a new species,

From what has been said it appears, that the mineral of Cape de Gattes differs from all other known substances. Its primitive form, specific gravity, property of transmitting rays of two different colours, and the other positive or negative characters, that distinguish it more particularly from each of the substances, with which it is most likely to be compared, are so discriminative, that we cannot avoid considering it decidedly as a new species, without recurring to the testimony of chemical analysis. At the same time it seems to me most suitable to give it a name from its remarkable property of double colour, and such is the etymology of the name, which Mr. Haüy had the goodness to suggest to me. I conceive myself sufficiently authorized to reject the denomination of yolite (violet stone), derived from the superficial colour of the crystals, because in the present case its application would be more inconsistent than in many others. Besides, it is too liable to be confounded with hyalite, appropriated to the concrete hyaline quartz,

and giving it the name of dichroït.

or

or yanolite, or yonolite, given by Delamétherie to the old violet schoerl.

It probably produces a double image.

From the primitive form of the dichroït it is to be presumed, that it possesses the property of producing a double image; but this I could not ascertain, for want of crystals sufficiently transparent. The verification of this conjecture however would be the more interesting, as the phenomenon of double refraction could take place only in a direction oblique to the axis of the prism; of which I have satisfied myself by experiment, and which the phenomena of the emerald sufficiently confirm. Hence we see, that, on the hypothesis of a double refraction, there would be such a relation between this phenomenon and that of the double colour, that the crystals would double images in the direction in which the colours appear mingled, while we should see a simple image by looking in that direction, in which each colour becomes exclusive.

XII.

Analysis of the Nadelertz of Siberia: by Mr. John*.

Nadelertz an ore of bismuth.

THE needle-ore has been considered in Russia as an auriferous ore of nickel. In the work of Reuss, and in the Ephemerides of baron Moll, it is classed among the ores of chrome: but the analysis of Mr. John, given in Gehlen's Journal, shows, that it is an ore of bismuth.

The following are its external characters according to Karsten.

Its external characters.

Its colour is steel gray, sometimes a pale copper red, or covered with a green and yellow coating†.

* Journal des Mines. vol. xxiv, p. 227.

† The yellow coating is so slight, Mr. John could only examine it by wiping it off with cotton moistened with nitric acid, washing the cotton in water, and evaporating the water and excess of acid It then appeared to him, from such experiments as he could make, to be oxide of uranium. The green coating, covering both the crystals and the quartz gangue, is thicker and more abundant. From his analysis it consisted of carbonate of copper, carbonate of lead, and bismuth.

Colour

Colour, where scraped, scarcely deeper than that of the fresh and shining ore.

It is disseminated, and crystallized in sixsided elongated prisms, accumulated in the form of needles. These crystals are sometimes curved, or jointed, always imbedded, and frequently crossing each other.

Their surface is striated longitudinally.

They have seldom any perceptible lustre on account of the coating. When this is wanting, their external lustre is but little. Interiorly it is always metallic.

Their fracture lengthwise is foliated, and very brilliant: transverse, uneven and brilliant.

Fragments, unknown.

Opake.

Feels smooth.

Soft.

Extremely heavy; its specific gravity being 6·125.

It is found in the mines of Pischminskoi and Klintzefskoi, near Ekaterinenbourg, in Siberia.

Its component parts, supposing the gold and quartz to be accidental mixtures, are

Component parts.

Bismuth	43·20
Lead	24·32
Copper	12·10
Nickel?	1·58
Tellurium?	1·32
Sulphur	11·58
Loss (oxigenized sulphur?)	5·90
	100·00

Patrin had considered it as an ore of bismuth long ago.

In a note subjoined to the preceding paper Mr. Patrin observes, that when he was at those mines in 1786 the needle-ore had just been discovered; and as its nature was not known to the managers of it, they made a secret of the particular spot where it was found. With some difficulty however he obtained a few fragments of it for their weight in gold. From such trials as the smallness of his specimens admitted, he considered it as a sulphuret of bismuth, by which name he described it in his Natural History of Minerals, published in January 1801, and reprinted in 1803.

Scientific

SCIENTIFIC NEWS.

Middlesex Hospital.

Medical lectures.

MEDICAL Lectures, 1810—11, by Richard Patrick Satterley, M. D. Fellow of the Royal College of Physicians, Physician to this Hospital, and to the Foundling Hospital: and Thomas Young, M. D. F. R. S. Fellow of the Royal College of Physicians.

Dr. Satterley's course of *Clinical* instruction will begin the first week in November: the attendance on the patients will be continued daily, and lectures will be given once a week, or oftener, when it may be necessary, at 11 o'clock. Mr. Cartwright, surgeon to the Hospital, will undertake such occasional demonstration of morbid anatomy, as may be required for the illustration of the respective cases. The objects of the course will also be extended to such remarkable peculiarities in the diseases of *children*, as may occur in the *Foundling Hospital*. Terms of admission, to pupils of the Hospital, five guineas.

Dr. Young will begin, in February, a course of lectures on *physiology*, and on the most important parts of the *practice of physic*; in particular the nature and treatment of febrile diseases; he will deliver them on Tuesdays and Fridays, at 7 o'clock in the evening. Admission, two guineas: to former pupils, one guinea.

Those who are desirous of attending either of these courses, are requested to leave their names with the apothecary at the Hospital, from whom farther particulars may be known.

Popular lectures on the sciences.

The annual courses of popular lectures at the Surry Institution, Blackfriars Bridge, commenced on the 15th ult., and will be continued every succeeding Monday and Thursday evening, at 7 o'clock, during the season. We understand, that the following gentlemen have been engaged for the respective departments, viz.

Zoology, George Shaw, M. D., F. R. S.—Music, Mr. S. Wesley.—Zoonomy, John Mason Good, Esq.—The chemistry

chemistry of the arts, F. Accum, M. R. I. A.—Natural philosophy and Astronomony, Mr. Hardie.

Mr. Singer's lectures on electrical and electro-chemical science will recommence early in the ensuing season, at the Scientific Institution, 3, Princes Street, Cavendish Square. In these lectures a complete exposition of the subject will be given, and the illustration will be assisted by some new and interesting experiments. A prospectus of the plan of instruction may be had at the Institution, or of Mr. Cuthbertson, 54, Poland Street.

Lectures on electrical and electrochemical science.

Mr. Barlow, of the Royal Military Academy, has ready for the press, an Elementary Investigation of the Powers and Properties of Numbers, with their application to the indeterminate and diophantine analysis, to which will be subjoined a synopsis of all the most curious problems of this kind, selected from the best ancient and modern authors.

Investigation of the powers and properties of numbers.

To Correspondents.

The paper of Messrs. Kerby and Merrick in our next.

METEOROLOGICAL JOURNAL,

For OCTOBER, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

SEPT. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
27	57°	60·5°	64°	53·5°	30·02	Fair	Fair
28	60·5	59	65	51	29·94	Ditto	Ditto
29	57	60	62	56	30·05	Ditto	Cloudy*
30	61·5	62	66	53	29·99	Ditto	Ditto
OCT.							
1	61·5	60	66	51	30·15	Ditto	Ditto†
2	57	55·5	62	49	30·25	Ditto	Fair
3	55·5	56	61·5	49·5	30·25	Ditto	Ditto
4	55·5	57	64	49	30·26	Ditto	Ditto
5	57	56	63	50	30·16	Ditto	Ditto
6	54	55·5	59·5	46	29·94	Ditto[a]	Heavy fog
7	50	56	59·5	49	29 95	Ditto[a]	Fair
8	54	58	59·5	52	29·97	Ditto[a]	Ditto
9	56·5	57	60	52	29·93	Ditto	Ditto
10	56·5	57	58·5	51	29·84	Ditto	Ditto
11	54·5	52	58	46	29·92	Ditto	Ditto
12	50	47	54	40	29·86	Ditto	Ditto
13	46·5	47	53	40	30·00	Ditto	Ditto
14	47·5	50	55	43	30·22	Ditto[a]	Ditto
15	49	52	54	42	30·19	Ditto	Ditto
16	47	54	54	51	29·91	Rain	Rain
17	56·5	58	60	52	29·66	Ditto	Ditto
18	56·5	55	60	45·5	29·38	Ditto	Fair
19	53·5	59	59	57	29·80	Ditto	Rain
20	59	52	61·5	49	29·74	Ditto	Ditto
21	54	61	61·5	44	29·67	Ditto	Ditto
22	55·5	55	58	46	29·33	Cloudy‡	Fair
23	50·5	51	54	42	29·67	Fair	Cloudy
24	48	47	52	39	29·72	Ditto	Fair
25	44·5	45	49	37	30·17	Ditto	Ditto
26	42	45	50	42	30·37	Ditto	Ditto
27	46	45	48	38	30·23	Cloudy	Ditto

* Rain in the night.
† Ditto.
‡ Boisterous day and night.
[a] Intervening fogs.

Rain 1·745 inch since last Journal.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1810.

ARTICLE I.

On the Electric Column. *By* J. A. De Luc, *Esq.* *F. R. S.*

Part III.

Concerning some Meteorological Phenomena, to the better knowledge of which it may lead as an Aerial Electroscope.

Necessity of circumspection in examining phenomena.

When, in our researches, we have in view some great and determined object, we are not only more assiduous in our endeavours to approach it, but more attentive not to be misled in the road, and less disposed to be satisfied with mere surmises, while we perceive that some real discovery may be obtained by more circumspection. I shall therefore explain first, why every new *electric* phenomenon, which we encounter in the course of our experiments, must be attentively pursued and analysed in itself, and not connected with gratuitous hypotheses; for fear of losing a thread, which might lead us in the labyrinth of the *physical causes* acting on our globe, among which the *electric fluid* holds a high rank; as will appear by the following great object concerning this *fluid*, on which natural philosophers have not yet sufficiently fixed their attention, though it is explained in my former works.

Lightning.

It is commonly supposed, that the *electric fluid*, which, under the form of *lightning*, darts from certain *clouds*, existed previously in them, ready to be discharged, at a proper distance, on bodies which possess less of this *fluid*, either other *clouds* or the *ground*. On this idea, not improbable at first sight, Dr. Franklin founded his invention of

Conductors.

pointed conductors elevated above houses, in hopes to preserve the latter from being struck by *thunder bolts*. With the above supposition, this method of security was very ingenious; for, if the *electric fluid* were *actually* accumulated in a *cloud*, ready to be discharged on the first part of the ground sufficiently elevated, a *pointed conductor* might discharge that *cloud* without a *spark*, as it does the prime conductor of an electric machine. But those who have frequently travelled on high mountains know certainly, that there is no analogy between a *thunder cloud*, and an *insulated body* on which *electric fluid* has been accumulated.

The electric fluid not accumulated in a thunder cloud.

A *cloud* is a mere thick *fog*, and thus such a completely *conducting* medium, that the most powerful electric machine worked in it could not, for an instant, accumulate the *electric fluid* on its *prime conductor;* it would be constantly diffused through that moist air, and lost in the surrounding bodies. This cannot be doubted; but it is supposed, that *clouds*, being surrounded by pure air, and thus insulated, can retain the *electric fluid* accumulated in them by whatever cause. In this consists the illusion, dissipated by what is observed on mountains. I have frequently been in valleys of the Alps, and of lower mountains, beset with *thunder clouds* leaning on both sides against *wet grounds*, and thus in so complete a *conducting* connexion with the mountains themselves, that it was impossible any accumulation of *electric fluid* could remain in the former; beside which, no cause of such an accumulation has ever been explained: however flashes of *lightning* were emitted from these *clouds*, with greater or smaller intervals, followed by the astonishing phenomenon of the *rolling of thunder*; and to suppose this to be the repetition of one *sound*, by *echoes* from *cloud* to *cloud*, is a fiction similar to that of poets or painters, who represent the gods as sitting on these fogs.

Lightning

Lightning and *thunder*, when considered in their true nature, and with all their associated circumstances, though they are the most striking, have remained till now the most obscure of the atmospheric phenomena; and as at the same time their production is evidently connected with all the causes acting in the atmosphere, that great laboratory of nature on our globe, beginning from the very formation of *clouds*, this obscurity is spread over all the terrestrial phenomena. It is certain, by what I have above explained, that an instant before a flash of *lightning* strikes our eyes, no accumulation of *electric fluid* could have existed in *clouds* leaning against *wet* grounds: the sudden manifestation of this enormous quantity of *electric fluid*, not existing before *as such*, must therefore be the consequence of some *chemical* operation, depending on some new cause, which either disengages it from some *combination*, or generates it by some *composition*; and being thus instantly set free, it rushes in a torrent, before it can be diffused in the *cloud* and through this in the *ground*. Beside this immediate consequence of the certain fact, that the quantity of *electric fluid* thus emitted did not, the instant before, exist as disengaged in the *cloud*, various other phenomena, attending this effect, prove the existence of some great successive chemical processes, manifested principally by the successive *detonations* forming what is called the *rolling* of *thunder*: these are undoubtedly produced by concomitant *decompositions* and *recompositions* of still unknown atmospheric fluids, some producing the decomposition of the *air* itself, others proceeding from this first operation, as shall be explained hereafter.

Thunder and lightning

the consequence of some chemical process.

This is one of the greatest objects, that could be offered to the attention of natural philosophers: for it must strike them, that no system on the nature of *air* and *water* can have any solidity, if it happens to be in opposition to these grand effects produced, under our inspection, in the great laboratory of nature: and though our observation has not yet extended to all the atmospheric phenomena necessary to be embraced for the discovery of their specific causes, yet it is sufficiently advanced to indicate, according to general known laws, these *decompositions* and *recompositions* of

Necessity of investigating atmospheric phenomena.

Atmospheric air not a mixture.

atmospheric air, as being a fluid *sui generis*, and not a mixture of two *aeriform fluids* differing in their nature, as has been concluded from specious phenomena produced in our experiments; but these phenomena I have explained in my works, without supposing such a mixture, in itself contrary to a number of atmospheric phenomena. This I shall here successively explain, though not with so many particulars as are contained in my works.

Rain.

Rain will be my first object; and indeed it ought to be so in every general system of chemistry, since no phenomenon, either spontaneous or artificially produced, is more connected with the manifestation of *water* in the modifications of *expansible fluids*; and none certainly is attended with greater consequences on our globe. With a view of supporting the new hypothesis of a certain *composition* of *water*, from which and its associate hypothesis of two distinct and defined *aeriform fluids* in the atmosphere, *rain*, so common a phenomenon, cannot be explained, the ancient and already exploded hypothesis of Mr. Le Roy, of *evaporation* being a *dissolution* of *water* by air, has been revived. This hypothesis, the only apparent resource of the modern theory of chemistry, was plausible at the time of its first publication, about 60 years ago, when meteorological observations were very little advanced; because it is certain, that *evaporation* restores, upon the whole, to the atmosphere, the same quantity of *water* as falls from it in *rain*, *dew*, and other aqueous meteors: but from a number of well determined phenomena, discovered by the progress of observation, this compensation is not immediate: that *water*, which ascends in the atmosphere by *evaporation*, passes through an intermediate state; undergoing a *chemical* transmutation, which makes it disappear to all our tests, sometimes for many months, it being then transformed into an *aeriform fluid*; and it must be by some inverse operation, that, all at once, *clouds*, *rain*, and the other concomitant phenomena are produced. I shall show hereafter how unfounded, as well as useless, is an hypothesis imagined for evading the consequences of these phenomena, which I have opposed to the new theory of chemistry; but first, I must proceed farther in the account of the phenomena themselves.

Evaporation supposed to be a solution of water in air.

but the water is changed into gas.

The

The above consequences may be deduced from the most common atmospheric phenomena, even when only viewed from the plain, provided they are observed in all their consequences; but it is on high mountains, the very region of *meteors*, that, from other circumstances not perceptible in lower situations, the observer is induced to wish for more knowledge in the astonishing operations performed in this laboratory. Such has been the case with Mr. de Saussure and myself, on account of our frequent visits to the mountains of our native country, for the geological pursuits in which we were engaged at the same time. The surprising phenomena concerning *moisture*, which we observed in these high regions of the *air*, led us separately to the pursuit and construction of our respective *hygrometers*; in order to understand, by experiments and observations with this instrument, in what really consists *moisture* in the atmosphere; and to follow certain of its modifications, as its sudden increase and diminution without perceptible cause: a knowledge which, if not leading to immediate discoveries on the other atmospheric operations, might at least clear the way to these discoveries by dispelling and preventing errours.

Meteors best studied among mountains.

Hygrometers.

When our experiments and observations were first published, they attracted much the attention of natural philosophers; but by degrees they have been forgotten, from the increasing prevalence of the hypothesis of a *composition* of *water*, to which they were opposed, in consequence of their connexion with the most common meteorological phenomena; an opposition explained even before this hypothesis was so much relied upon as to effect a change in the whole nomenclature and language of chemistry.

Observations at variance with the supposed composition of water.

This inattention, for a time, to real and important discoveries, an effect occasioned by prevailing prejudices, is observed under various forms in the History of Sciences; but there it is seen also, that an obstacle of this nature could not be perpetual, and it may be expected, that it will not be so in this case; therefore I shall here assemble some uncontroverted results of observation and experience, for the consideration of natural philosophers.

Article I. *Evaporation*, the original source of *atmospheric phenomena*, is not a *dissolution* of *water* by *air*, as is now so commonly

Theory of evaporation.

commonly assumed; *air* has no share in it. The immediate product of *evaporation* in all its stages, from the formation of *steam* by *boiling water*, down to the *evaporation* of *ice* in winter, is constantly and uniformly an *expansible fluid*, composed of *water* and *fire*, namely the *aqueous vapour*. This *fluid*, in whatever *temperature* it is produced, acts by *pressure*, in the same manner as the *aeriform fluids*, and in particular on the *manometer*, from the instant of its production, as long as it subsists; and the quantity of its production, attended with a proportional *pressure*, is the *same* in *vacuo* as in *air*, at its different *maxima* correspondent to each degree of *temperature*; a direct proof that *air* has not the smallest share in *evaporation*. Lastly, as long as this *fluid* subsists without any change in its nature, it never ceases to act upon the *hygrometer*, and its quantity is exactly measured by this instrument, with the addition of the *thermometer*. I have proved these assertions by the union of Mr. de Saussure's experiments and mine, in some papers published in the Phil. Trans. of 1793. It is evident, that, if these be real *facts*, the resource of the new theory of chemistry for explaining *rain* is overturned (as will be seen hereafter), and with it the theory itself; what then is the reason, that those, who still maintain it, remain silent on these *facts*? On this however rests (and will continue to rest till the contrary be proved by direct experiments) the whole of meteorology.

The maximum of aqueous vapour at every temperature a fixed quantity.

Art. II. Both Mr. de Saussure and myself have determined, by direct experiments related in our respective works, as I shall more particularly express hereafter, the *quantities* of *evaporated water* contained in one cubic foot of *air* correspondent to every degree of our *hygrometers*, at every *temperature*; and we have proved, that the *maximum* of this *water*, a *quantity* fixed for every *temperature*, cannot be exceeded, either by the increase of *water* in the same space, or by the diminution of *heat* with the same quantity of this *water*, without some of the *aqueous vapour* being decomposed, and *water* making its appearance by *precipitation*: and by my experiments it is moreover demonstrated, that no length of time, after the production of this *fluid*, can prevent either its effect on the *hygrometer*, or its remaining

submitted

submitted in the same manner to the influence of *temperature*.

Art. III. The *aqueous vapour*, i. e. the immediate product of *evaporation*, is therefore never concealed in the atmosphere; and its *quantity*, in any part of the latter, can always be determined by the observation of the *hygrometer* and the *thermometer*. This *fluid*, produced by the *evaporation* that never ceases on the surface of the water and of the land, being of a specific gravity less than that of *air*, constantly ascends in the atmosphere, passing through its lower regions, where we do not find that it remains; it ought therefore to accumulate in the higher parts. Now, as we ascend on mountains, the *hygrometer* indicates less and less *evaporated water* in the transparent *air*. I shall soon answer the hypothesis, already mentioned as having been imagined for setting aside the conclusion which I have deduced from this phenomenon, namely, a transmutation of the *aqueous vapour* into *atmospheric air*; a conclusion however which will be found the ultimate result of this series of facts.

Aqueous vapour is always perceptible by the hygrometer.

Ascends,

but diminishes as it ascends

by conversion into air.

Art. IV. Another phenomenon, which Mr. de Saussure and myself have observed, proves, that *dryness* is still greater in the region of the atmosphere above the highest mountains, where it was natural to suppose, and I supposed it at first, that the *aqueous vapour* was accumulating. On plains and small hills, *moisture* is increasing in the *air* after sunset; and before we possessed our *hygrometers*, we had reason to suppose, that it was the same upon high mountains, for there also the *grass* becomes *wet*. This being the first common symptom of *moisture* observed after sunset, and even before, was one of the arguments in favour of the idea, that *dew* proceeds from the *ground*; but the *hygrometer*, that neglected inftrument, has shown it to be a phenomenon belonging to the physiology of plants, and not to meteorology. On high mountains, while the *grass* on the ground becomes *wet*, the *hygrometer* being suspended at some height above the ground, in some insulated spot where the *air* is free, shows an increase of *dryness*, which continues during the night. I have determined the cause of this phenomenon by immediate observations; it proceeds from the condensation of the columns of *air*, while the *heat* diminishes in them; whence

Dew belongs to the physiology of plants.

Air on mountains driest in the night.

whence results, that the part of that *air*, which, during the day, rested on the *summits* of mountains, descending lower, is followed by the *air* which was higher before; and this, as long as the condensation continues in the lower parts, descending from higher regions, and thus passing over the *summits* in its way downwards, is found, in an increasing degree, *drier* than that which rested on them in the day.

Theories of dew.

Art. V. Among the atmospheric phenomena, that of *dew*, commonly considered as very simple, has been long, and is still now, an object of controversy among natural philosophers, who have not attended to the latest experiments and observations. The first and most plausible explanation was, that the *dew* descended from the *air*, by the condensation of the *evaporated water* spread in it, when *heat* diminishes; but some experimental philosopher, finding that this cause was not sufficient to explain all the circumstances of *dew*, conceived the idea, which I have above mentioned, that it ascends from the *ground*, because this retains longer the *heat* of the day, than the *air* above it; which circumstance was considered as increasing *evaporation*: both parties alleging in support of their opinion certain facts, which, though not denied, were not decisive. During the most active time of this controversy, about 60 years ago, I made with my brother various kinds of experiments and observations, which, by turns, favoured one or the other of these hypotheses, but neither of them decisively; and the question would have remained for ever in suspense, had not *hygrology* and *hygrometry* been pursued with the degree of attention and labour, that Mr. de Saussure and myself have bestowed upon them; from which the phenomenon of *dew* has appeared under a new and quite different aspect, which excludes both the above causes as fundamental in it, and shows why neither of them could explain its most essential circumstances.

Agreement of the experiments of Mr. de Saussure with the author's.

Art. VI. With respect to the experimental part, we have both determined, by direct and unconcerted experiments, the effects produced on our respective *hygrometers*, placed in a mass of *air*, wherein, the quantity of *evaporated water* remaining the same, there was no change but in the degree of *heat*. We have made the same kind of experiments on different

different quantities of *evaporated water* in the same *space*; and combining them, we have formed *tables* expressing the different effects of *heat* on *moisture*, correspondent to different quantities of *evaporated water* in the same *space*, and to the changes of *heat* in each of these quantities; from which *tables*, after having observed the *hygrometer* and the *thermometer* in any part of the atmosphere, the quantity of *evaporated water* contained in one cubic foot of that *air* is determined. These entirely distinct experiments have proved the constancy of the laws prevailing in these effects, by the astonishing agreement of our *tables*, though determined by very different instruments and processes: an agreement which I have shown in the already mentioned papers to the Royal Society.

Art. VII. This determination of the effect produced on *moisture*, i. e. on the indications of the *hygrometer*, by the changes of *heat*, in a mass of *air* wherein the quantity of *evaporated water* remained the same, was most essential in meteorology; and in particular it was indispensable for the decision of the question, whether the production of *dew* were principally owing to the *cooling* of the atmosphere; which appeared the most natural explanation, but on which however there were sufficient reasons of doubt to produce the obscurity which remained on this phenomenon; because nothing could be either determined or proved, concerning the real effect of the diminution of *heat* on *evaporated water*, without such experiments as above defined; and I come now to their immediate application to the phenomenon of *dew*; in consequence of some observations which were also separately made by Mr. de Saussure and myself. Towards *sunset* and in the beginning of the night, *moisture* increases in the *air* much more rapidly; and after *sunrise* and in the first part of the day, *dryness* increases also much more rapidly—in both cases, comparatively with the correspondent changes of *heat*—, than would be the case, did the same quantity of *evaporated water* remain in the *air*. This is a very succinct account of our experiments and observations concerning this object, the particulars of which may be seen in our respective works; but it is sufficiently distinct to allow me here to conclude, that thus has been pointed out

Aqueous vapour in the air diminishes in the morning and increases in the evening.

one

one of the greatest questions and objects of investigation, concerning *terrestrial physics*, namely: what is the *cause* of the *disappearance* in the atmosphere of the greatest part of the *aqueous vapour* which it before contained, when the *sun* ascends on the horizon; and of the *increase* of its quantity, when the *sun* is setting; while the very reverse should have been expected from all the hitherto known causes? (as I shall show hereafter). To this investigation I shall now proceed as far as known phenomena will lead me.

Changes in the electric state of the atmosphere observed by Mr. de Saussure.

Art. VIII. I shall first mention a very important course of observations of Mr. de Saussure concerning the changes in the *electric state* of the atmosphere. He had erected a high *conductor*, in a favourable situation, on the brow of a hill in Geneva. The lower part of this *conductor* was connected with an insulated pair of pith *balls*, the divergences of which indicated the *differences* between the *electric state* of the upper air, and that in which the *balls* stood: he observed during many years the diurnal variations of this *difference*; and the main result of these observations is the following. In common weather, i. e. when no particular cause disturbs the course of the usual operations going on in the atmosphere during each period of 24 hours, the quantity of *electric fluid* increases in it from *sunrise* till some time in the afternoon; as is seen by the increase of a *positive* divergence of the *balls*. The new *electric fluid*, the formation of which is thus indicated, accumulates in the air, because it is transmitted but slowly to its lower part near the ground. But afterward, when the *hygrometer* shows a beginning of increase of *moisture* in the atmosphere, the divergence of the *balls* begins to decrease; and when at last *dew* is forming; the *electric equilibrium* is soon established between its upper and lower parts, the whole of the *electric fluid* formed in the day passing then into the ground. Now, it is during the first of these periods, that *dryness* increases in the atmosphere much more than would happen by the same increase of *heat*, did only the same quantity of *aqueous vapour* subsist in it as before *sunrise*; while on the contrary its quantity ought to increase by a greater *evaporation* being produced on the ground, which *dries* when heated by the sun. Hence it appears, that there is some connexion be-

The diminution of electric fluid in the atmosphere connected with the decrease of aqueous vapour.

tween

tween the increase of the quantity of *electric fluid* and the diminution of that of *aqueous vapour* in the atmosphere, during this period.

Art. IX. This points out, in the first place, a *formation of electric fluid* in the atmosphere, while the *sun's rays* pervade it. *Light*, the increase of which in the atmosphere is here the immediate cause, is certainly one of the *component* parts of the *electric fluid*; therefore, this *fluid* must be *composed* in some operation of nature on our globe. Now it is here already probable, that the *sun's rays*, in pervading the atmosphere, encounter in it the substances with which they compose the new quantity of *electric fluid* then manifested: and that, in general, they enter there into various combinations, is proved by their intensity being sensibly greater on the top of high mountains than in the lower parts of the atmosphere, as has been shown from experiments by Mr. de Saussure; which difference must proceed from their quantity being diminished in pervading the air.

The electric fluid increases with the presence of the sun.

The solar rays more intense on heights.

Art. X. Some other experiments of Mr. de Saussure lead besides directly to this system concerning *compositions* and *decompositions* of *electric fluid*, as producing phenomena, the causes of which were unknown or mistaken. For instance, it has been found by experience, that, when water is poured upon an insulated plate of hot iron connected with an electroscope, this plate becomes *negative:* whence it had been concluded, that, when *water* is converted into *vapour*, it acquires a greater capacity for *electric fluid*; and thus deprives of a certain quantity of this *fluid* the body on which it evaporates. But Mr. de Saussure, having repeated the same process upon different heated bodies, found, that some, in particular silver, became *positive:* whence he concluded very naturally, that during the evaporation of water on hot iron some *electric fluid* was *decomposed*, and some on the contrary *composed* when the same operation took place on silver. He has also surmised, what I have since found by direct experiments related in my work *Idées sur la Météorologie*, that in the discharge of the *Leyden vial*, and in my experiments of the *magic picture*, the *spark* produces some diminution of the quantity of *electric fluid* on these *bodies*; which cannot be but by *decomposition*. It will successively

Farther instances of the effects of the composition and decomposition of the electric fluid.

cessively be seen in what manner these previous remarks on the *electric fluid*, and the experiments on the same subject contained in my former paper and the first parts of this, are connected with meteorology.

Region of clouds and rain.

Art. XI. I have said, that, as we ascend mountains, the *hygrometer*, successively falling, indicates less and less *evaporated water* in the *air*. We thus however attain the region, in which *clouds* and *rain* are formed; and there it is, that the lessons of nature itself may guard us against the arbitrary dictates of imagination: I shall therefore relate what I have observed. At times when the atmosphere is so clear, that distant objects are seen very distinctly, and that the *hygrometer*, according to the tables that Mr. de Saussure and I have made from direct and separate experiments, does not indicate above two or three grains in each cubic foot of that *air*—small *clouds* may be seen forming in all parts of the very stratum of the atmosphere in which we stand, with very little or no *wind*. Sometimes, without any change in the *temperature* or *moisture* of the intermediate parts, these embryoes of *clouds* dissipate: but at other times they rapidly increase, unite together in the whole stratum in sight, and announce to the observer, that soon he will be enveloped by *clouds*. However, till the *clouds*, either moving towards him, or forming around him, occupy the very spot in which stand the *hygrometer* and the *thermometer*, he observes no sensible change in them: but the instant that a *cloud* envelopes him, the *hygrometer* arrives at its point of *extreme moisture*, and all the bodies are *wet*.

Formation of clouds.

Rain,

Art. XII. These preliminaries of *rain* often remain a long time, with only some variations, and at last dissipate without effect; and as soon as the *clouds* disappear in one spot, the *hygrometer* indicates the same *dryness*, as if no *cloud* had been there. But at last; though without any perceptible difference in the preliminaries, because some other *test* of the state of the *air*, beside those we possess, is *wanting*; the *clouds* increase in extent and thickness, above and below the place of observation, and *rain* is produced in more or less abundance. If *rain* be lasting, and at the same time in a great extent of country, it may happen either in a calm air, or during some regular *wind*. But when *rain* is partial

with wind.

and

and in *showers*, sudden, and sometimes violent *winds* accompany these, arising from the *expansion* of the *air*, by its decomposition into *aqueous vapour*, in some place, while a *vacuum* is produced in other parts, by the resolution of that *vapour* into *rain*. Hence it is, that the direction of these *winds* is rapidly changing, and that they cease with the return of the transparency of the *air*. Lastly, in a *stratum of air*, which perhaps only half an hour before was *calm* and *transparent*, in which the *hygrometer* did not indicate any increase in the small quantity of *evaporated water*, and without any indication of increase of the quantity of *electric fluid*, some *clouds*, rapidly forming, produce *lightning*, *thunder*, *hail*, torrents of *rain*, and such violent *winds*, as tear up trees and overturn cottages on mountains. Storms.

We may be for ever ignorant of the causes of these wonderful phenomena, but those who are aware that fiction, in the operations of nature, may lead to great errours, will prefer ignorance to a false science. As for me, from my first observations of these operations of unconstrained nature, and with the addition of a remark of Mr. de Saussure which I shall mention, I changed very essentially my former ideas on the atmospheric phenomena, as I have explained in my works, and shall repeat hereafter.

Art. XIII. In order to evade the general consequence, which, in my works, I have deduced from these facts, namely, that *rain* and the other concomitant phenomena are produced by different kinds of *decompositions* of the *atmospheric air*; which consequence is certainly the subversion of the new theory of chemistry; Mr. Fourcroy invented the hypothesis of a *dry* solution of *water* by *air*; supposing, that this *water* could no longer affect the *hygrometer*, which in consequence he discarded from the rank of a *meteorological instrument*; and having obtained the assent of many chemists, who have not applied to meteorology any more than himself, this instrument, so much wished for before by natural philosophers, is now hardly mentioned. Fourcroy's hypothesis of a dry solution of water in air.

But this hypothesis, grafted on that of *Le Roy*, is in the first place absolutely *gratuitous*; no fact having been adduced in bringing it forward in chemistry against the positive facts contained in Mr. de Saussure's works and mine:

and

and besides it is of no avail, since Mr. Fourcroy himself, and all those who have adopted it, have been obliged to suppose, that this pretended *solution* of the *water* remains dependent on the *temperature*; which they are obliged to do, otherwise it would be nothing more than my system, with the appearance of refuting it. For, if the enormous quantity of *water*, which sometimes falls in *rain* from a very limited stratum of *air*, be not submitted to *precipitation* by the diminution of *heat*, it must have been changed into a *permanent* or *aeriform fluid*; and in the atmosphere no sensible quantity of any *fluid* of this kind exists, but the *atmospheric air*. Besides, since for this reason it is supposed in that hypothesis, that the *evaporated water* remains dependant on *temperature*, very little knowledge in hygrology is required to conclude, that it cannot cease to affect the *hygrometer* in proportion to its quantity, as is evident from Mr. de Saussure's experiments and mine. Lastly, with respect to that *fluid* the *decomposition* of which produces *rain*, its nature is clearly determined by the following circumstance: when we remain in a stratum of *air* till the end of the operations by which a deluge of *rain*, even with *lightning* and *thunder*, has been produced, the *residuum*, according to all tests, is the same *air*, as before. Such are the objections which I have made to Mr. Fourcroy himself, and which he has not answered, or any chemist for him.

Storms do not alter the nature of the air to any chemical test.

Art. XIV. These formations and modifications of *clouds*, when viewed only over head from the plain, have naturally inspired the idea, that by some cause the liberation and condensation of *evaporated water* now and then take place in a great extent of the upper region of the atmosphere, which *water* descends and accumulates in the stratum of *air* where *clouds* form and produce *rain*. But this idea proceeds from a want of previous knowledge in hygrology, and of observations on high mountains: for in the first place, whencever, and from whatever cause that quantity of *water* may be supposed to proceed before any *precipitation* can take place, even in the first state of *vesicular vapour* which constitutes *clouds*, it must be preceded by *extreme moisture* in the still *transparent* air, since it is only the *excess* of that *water*, which is first precipitated in a *mist*; and when this

Deception from observations on plains.

precipitation

precipitation ceases, *extreme moisture* still subsists in the air; as Mr. de Saussure and myself have found in all our hygroscopical experiments. Now, I have said above from observations on high mountains, that air is there *dry* till the moment before the formation of *clouds*, and that as soon as the *clouds* are dissipated, the *hygrometer* indicates the same *dryness* as before. This evidently shows, that the production of *clouds* and *rain* have their *cause* in the very *stratum* of *air* where they are manifested; and this *cause* cannot be any other than a *decomposition* of the *air* itself.

Clouds and rain from decomposition of the air.

Lastly, in these very *clouds*, which, being themselves a *conducting* mass, lean besides against mountains, it happens sometimes that *lightning* and *thunder* are produced; and this, as I have said before, without any previous sign of an uncommon quantity of *electric fluid* in them. This also points out some operation taking place in these *clouds*, by some modification in the cause which commonly produces a simple *rain*. The *electric fluid*, thus suddenly disengaged, must have been before in some chemical combination in the *air* itself, which prevented its manifestation, and is then destroyed. When we are above the *clouds*, we may see (as it has happened to me) *lightning* darting upwards, as it is commonly seen darting downwards when we are under the *clouds*; and even in this last case, we may judge that *lightning* is darted upwards, when we see only a great sudden light in *clouds*, without any *flash*, followed however by *thunder*.

Thunder and lightning.

The above are leading facts in the maze of atmospheric phenomena, certainly indicating the existence, in the atmosphere, of *subtile fluids*, beside those which have hitherto been discovered. This is the general object, which I am going to examine.

These facts evince the existence of subtile fluids in the atmosphere.

I shall here begin by explaining one of the results of my long labours in the pursuit of the *measurement* of *heights* by the *barometer*, of which all the steps are described in my work *Recherches sur les Modifications de l'Atmosphère*, published in 1772. My experiments and observations were first directed toward these two points. 1. To obtain, by a great number of observations at different measured *heights*, on mountains and towers, a *coefficient* expressive of *height*, to the

Investigation of the measurement of heights by the barometer.

the determined *law* of the *densities* of *air* correspondent to the differences of *pressure*, in a given *temperature* of the *air*. 2. An *equation* for the differences of actual *temperatures* with that fixed point. By dint of trials, I arrived so far in these determinations, that the method of measuring the height of mountains by the barometer has been found preferable to geometrical operations, on account of the impossibility of determining a law in the *terrestrial refractions*; beside the difficulty of finding proper bases for the triangles. But though the measurement of heights had been my first view in this undertaking, other modifications of the atmosphere became soon predominant in my pursuit, as I shall now explain.

In order to ascertain the degree of exactness, which could be obtained in the determination of the two parts of the *formula* above defined, I had measured the heights of 14 points above one another on Mount *Salève*, near Geneva; the whole height, above the level of the base that I had chosen, being above 3000 feet; and at each of these determined elevations I had made a great number of observations of the *barometer* in different temperatures, both in the same days and in different seasons. I had taken all possible precautions to ascertain the height of each of these points, in verifying the trigonometrical operation by levelling the whole mountain in passing by these points, in order that the *formula* might be applicable to other places: but had there been some inaccuracy in that respect, it could not affect the *coefficients* of the two *laws*, as applied to the same places; for if these *laws* had been sufficient, the *formula* would have assigned to them the same *height* by every observation.

Anomalies apparently from atmospheric phenomena.

Now, no *coefficient* to the *differences* of heights in the *barometer*, associated with any *equation* for the *differences* of the *thermometer*, could bring the *formula* to express the same *differences* of *height* between the same *points*: a proof, that the *two conditions*, with which alone the *formula* corresponds, do not embrace all the *causes* of variation in the *density* of *air*. Having however no other *data*, I fixed these two parts of the *formula* in the manner the most correspondent to the whole of my observations amounting to near 600; so that the *sums* of *anomalies* in *plus* and *minus* comparatively

comparatively to the measured *heights* were equal; after which the *causes* of these *anomalies* became the object of my researches.

Expansion by heat not sufficient to account for these.

One of the principal means by which I had considerably reduced the former great *anomalies* in this measurement, which had appeared unconquerable, had been by introducing an *equation* for the differences of *expansion* of air produced by heat in the atmosphere. Considering therefore this effect of *heat*, which, by its increase, diminishes the *pressure* of columns of *air* of a determined *height*, under the same *pressure* of superincumbent air, as indicated by the height of the *barometer* at the upper station; and connecting this circumstance with the idea, that the *cause* of *heat* is an *expansible fluid*, namely *free fire*, which occupies in *air* a *space* without any sensible addition to its *mass*; I concluded, that some other *fluid*, for which we had not yet a *test*, as we have by the *thermometer* for the former, might be the cause of the above remaining *anomalies*.

The presence of aqueous vapour suggested itself;

This general conclusion brought into my mind the *aqueous vapour*, of which I knew that the *specific gravity* was much less than that of *air*; and supposing at that time, as is commonly thought, that its accumulation in the atmosphere was the cause of *rain*, I conceived that the difference of its quantity in different times must be very great, and that this might be the cause, or at least one of the causes of the *anomalies* I had in view.

and the state of the fixed barometer.

The same consideration led me also to a system concerning the remarkable, though not constant, correspondence of the *variations* in the sedentary *barometer* with *rain* and *fine weather*; as the same *fluid*, the abundance of which in the atmosphere diminishes its *pressure* upon the *barometer*, was supposed to produce rain. Having published this system in the above mentioned work, it obtained much approbation among natural philosophers, because no satisfactory explanation had been yet given of the above connexion of phenomena; however I did not intend to give it a full assent, till I had succeeded in the construction of a true *hygrometer*; judging already, that, without such an instrument, nothing could be determined with any certainty, concerning the modifications of *evaporated water* in the atmosphere. This

judgment was soon confirmed; for Mr. de Saussure, who had made a quicker progress than myself in hygrometrical experiments, discovered the fallacy of the above plausible system, which at first he had adopted with applause: he demonstrated by direct processes, that, though the *aqueous vapour* is specifically *lighter* than *air*, the difference between its greatest and smallest quantity in the atmosphere at any time is so little, that it can explain but a very inconsiderable part of the *variations* of the *barometer*. I had not yet carried my own experiments so far, but I did not doubt the main result of his, as they bore all the characters of a true inquiry, and I abandoned my system, as applied to the *aqueous vapour*.

Aqueous vapour inadequate to account for the variations of the barometer.

But the general conclusion that I had deduced from the great reduction of *anomalies* in the measurement of heights by the barometer, which was principally owing to the introduction of an *equation* for the differences of the quantity of *free fire* in the air, still remained; namely, that some *expansible fluids* as *imponderable* as the former, hitherto unknown to us, might also account for that remarkable correspondence between the *changes* of the *weather*, and the *variations* of the *barometer*. For, the same *fluids*, which, from their abundance at certain times, lessen the pressure of the atmospheric columns on a certain extent of country, by dilating the *air* and repelling it to other parts, may also prepare its *decomposition*, for the production of *rain*, either alone, or accompanied with other *meteors*; and at other times they may be dissipated without producing any of these effects, occasioning only the fall of the *barometer*.

Probably other imponderable fluids in the atmosphere beside free fire.

The above series of facts and their immediate consequences present the greatest assemblage of operations of physical causes on our globe; and a general consequence certainly results from them, namely, that all these operations are so intimately connected with the nature of *aëriform fluids*, of *water*, *light*, *fire*, and *electric fluid*, that we cannot determine any thing with the smallest degree of certainty on the nature of any of these substances, without embracing the whole. When therefore we discover some new phenomenon of any of these *fluids*, at what distance soever this phenomenon may be from connecting itself with the

Meteorology intimately connected with the nature of the component parts of the atmosphere.

the operations which we observe in the atmosphere, it is not to be neglected; for we cannot arrive at any distant object, but by successive steps.

Hence attention to be paid to the aerial electroscope.

This is the consideration, that has induced me to fix my attention on the *electric* phenomena manifested by the instrument, which I have described under the name of *aërial electroscope*: as from the above atmospheric phenomena concerning *lightning* and *thunder*, which cannot leave any doubt, that they are produced by a certain *decomposition* of the *atmospheric air*; and from the correspondent circumstance of a formation of new *electric fluid* in the atmosphere, during the period of the day, when, the greatest part of the *aqueous vapour* vanishing in it, there remains scarcely any *ponderable* fluid but *atmospheric air*; it is manifest, that the *electric fluid* is one of the substances most intimately concerned in the *chemical* processes, which take place in the atmosphere, and on the nature of which it is the most important to acquire more knowledge.

Its movements unconnected with heat or moisture.

It may be seen in the *tables* of my observations of this new instrument, that the changes in the frequency of the *strikings* of the little *pendulum* have no determined connexion with those of either *heat* or *moisture* in the room; for though *heat* commonly increases in the course of each day, at the same time as the *frequency* of the *strikings*, nevertheless the former is not the cause of the latter; since with the same degrees of *heat* the frequency of the *strikings* is very different on different days. With respect to the correspondence of this phenomenon with the *variations* of the *barometer*, my observations have been too short for deciding any thing on this object, though I felt much interested in it; and besides, the *barometer* had but small variations during this short time, being always rather high. Therefore, this is a course of correspondent observations which remains to be followed.

Variations in the electricity of the ground and air.

The observations contained in the last *table* create a new interest in this pursuit, as they may become a mean of discovering the changes in the comparative *electric states* of the *ground* and the *air* near it. The little *pendulum*, by its *silver wire*, being placed in connection with the *zinc* side in these experiments, was therefore *positive*; and in this case

(as well as when it is connected with the *copper* side) it must rise more rapidly towards the *ball* 18, in proportion as the *electric state* of the latter differs more from its own. We know that in the first case (that of my observations), when the *ball* 18 communicates with the *copper* extremity of the *columns*, it is *negative*, and thus differs from the *pendulum* as *negative* from *positive*; the *standard* of which, according to the important determination of Sig. Volta, is the *actual* electric state of the *ambient air*. Now, the observations contained in the above *table* show, that the *frequency* of the *strikings* is not always the greatest, when the *ball* 18 is undoubtedly *negative* by communicating with the *copper* side of the *columns*; it being often equal, and sometimes even greater, when the *ball* communicates with the *ground*. This is a remarkable phenomenon, showing, that sometimes the *ground* contains less *electric fluid* than the *air* near it, and it may in future lead to some important discovery concerning the operations going on at the surface of the ground depending on the atmosphere.

Sometimes the ground contains less electricity than the air near it.

I have employed much time and labour, to arrive at the entrance of this new road in the investigation of terrestrial phenomena: the *entrance*, I say, for I do not even consider it as completely open. With respect to the instrument itself, I may judge, that it is susceptible of farther improvements, both in the composition of the *column*, and in the machinery added to it: for in such a complication of new physical effects and mechanical dispositions of parts, it is not to be expected, that every thing can be conceived by one individual. The very composition of the *column* might be improved with regard to the intensity of effect, by some other *metallic* coating than that of *copper* on *paper*, which I have employed on account only of its being ready prepared by using *Dutch gilt paper*. In some trials, I have found more effect in using *paper* covered with real *gold*, and with *silver*; and I have also found some advantage in doubling the *Dutch gilt paper*, by pasting a thin paper over its own paper. Many such trials may be made with a proper *condenser*, before whole *columns* are composed. As for the arrangement of the machinery connected with the *column*, the instrument which I have described having been successively

This path but just entered.

The instrument may be improved.

Remarks on the column.

successively augmented upon its original base, I suspect that it is too much crowded, and that thus its parts may have on each other an influence prejudicial to the effects, which I have remarked in other cases.

I have made many other remarks on this instrument, but my present purpose is more to engage other experimental philosophers in this pursuit, than to forward it myself; for with respect to these observations, I consider them as newly born. It will first require some time for the understanding what may be called the *language* of the instrument; i. e. its meaning as to the indication of the *electric state* of the *ambient air*, by its influence on the motions of the *pendulum*. This study has been opened too late for me, though I was engaged in it by considerations resulting from long *meteorological* observations, which, as they are of the greatest importance to natural philosophy, must be the incitement to this pursuit. Wishing therefore, that such observations may become a more general object of attention among natural philosophers, I have here endeavoured to show, by an abstract view of their present results, what knowledge, in following them, may be still obtained concerning the atmospheric operations. It is true, that observations of this kind require the neighbourhood of mountains (unless those who ascend in balloons should carry proper meteorological instruments, and apply themselves to these observations). But in general no real knowledge of the *nature* of the *atmosphere* can be obtained without, in some manner, ascending in it; and it is no less certain, that without this knowledge, no *chemical theory* can possess any certainty.

The indications of the instrument to be studied.

High parts of the atmosphere should be examined.

Systems are useful for promoting science, provided they be founded on all the knowledge already acquired respecting their object; but even then, as long as they contain *hypotheses*, they must be only considered as leading to new researches on determined points. With this view, I shall here conclude by an abstract of a meteorological system which I have fully explained in my former works, and especially in that under the title of *Introduction à la Physique terrestre par les Fluides expansibles*.

Systems useful.

1. During

Aqueous vapour transformed into atmospheric air by electricity.

I. During the time that the *sun's rays* pervade the atmosphere, the *aqueous vapour*, ascending in it by the *evaporation* which continually takes place on the surface of the earth, is transformed into *atmospheric air*, by some combination of this *vapour* with the *electric fluid*, which, during the same time, is formed in the atmosphere. A formation of *electric fluid* at that time is shown by Mr. de Saussure's observations already mentioned; but that the quantity thus manifested is not the whole, but that a great part of this new *fluid* is employed in the above transformation, is proved, as will be seen hereafter, by the production of *lightning* and *thunder*, which cannot have any other source.

Vector.

II. Thus, but by a particular operation, is formed that subtile *fluid*, which I have called *vector*, possessing many of the properties of light, but with the characteristic differences which I have determined. This fluid pervades instantly all bodies, is constantly present in the atmosphere, and has probably a great share in its phenomena; but its only function yet determined is, to unite with the *electric matter* composed at the same time; and, being thus the cause of the expansibility of the *electric fluid*, it produces all the phenomena known under the name of *electric influences*, as I have explained.

Dew.

III. In clear weather *dew* is produced at *sunset*, because that formation of *electric fluid* then ceasing in the *atmosphere*, the *aqueous vapour*, which continues to ascend in it, remains unchanged, and its quantity increasing too much in the air comparatively to the decreasing *heat*, it precipitates in visible particles of water: when *heat* decreases very rapidly in the air after *sunset*, the *vapour* is seen condensed as a *mist* over meadows; and at last in autumn it produces *fogs*.

Mist, and autumnal fogs.

Clouds and rain.

IV. The return of *atmospheric air* into *aqueous vapour*, whence result *clouds*, and afterward *rain*, is produced by some *subtile fluid* ascending from the base of the atmosphere, the *affinities* of which with the ingredients whereby the *aqueous vapour* has been transformed into *atmospheric air* decompose the *latter*. Thus, particles of *aqueous vapour* being substituted for particles of *air* in some *stratum* of the atmosphere, and becoming much too abundant to subsist in the same

same space, they first *precipitate* in the *vesiculæ* which form *clouds*; and if the *decomposition* of the *air* continues some time in the same *stratum*, these *vesiculæ* collapse into *drops*, and form *rain*.

V. This is one of the causes of the *variation* of the *barometer*, not as a *prognostic*, but as a *consequence*. The absolute *mass* of the atmosphere is constantly changing by these inverse operations. When there is a long duration of *fine weather* over a great extent of country, the absolute quantity of *air* increases in the atmosphere, by the *aqueous vapour* which ascends in it continuing to be transformed into *air* during the day; and the *barometer* ascends, even in parts at some distance where it *rains*: when on the contrary there prevails over a great extent of country a long continuance of *decomposition* of *air* into *rain*, the mass of the atmosphere decreases, and the barometer falls, even in adjacent countries where there is *fine weather*. It is not therefore to be expected, that *rain* and *fine weather* should be positively connected with certain *absolute* heights of the *barometer*; its small motions, when it is more or less high, have the surest correspondence with the local weather; the fall indicating the presence of that *subtile fluid*, which tends to *decompose* the *air*, and the ascent, the cessation of this influence. Variation of the barometer.

VI. If, during the *decomposition* of *atmospheric air*, the *fluid* operating this effect so unites with the ingredients of the *electric fluid*, which had entered into the composition of that *air*, as to form a new *compound* in which the *electric fluid* does not possess its characteristic properties, *rain* only is produced, with little or no *electric* symptom; and this is the most common case. But when, from the nature of the new *fluids* which come to be spread in that stratum of the atmosphere, the decomposition of *atmospheric air* is such as to permit *electric fluid* to be produced by the precise ingredients (i. e. neither more nor less) necessary to its characteristic properties, it darts suddenly into the air in *lightning*: but this is only a first effect, and not yet *thunder*, a most astonishing phenomenon, consisting undoubtedly in successive *detonations*, such as the report of cannons fired in a rapid succession; and the former detonations must have with Rain alone. Lightning. Thunder.

with the latter this analogy of cause, that they are *explosions* of a particular *expansible fluid*, produced by that kind of sudden decomposition of *atmospheric air*, as it happens by firing gunpowder and other processes.

Hail. VII. A direct proof of these sudden *decompositions* of some substances in such *clouds*, and simultaneous *compositions* of other substances, is the production of *hail*. This effect shows, that, in a certain combination of circumstances, such a quantity of *free fire* enters suddenly into some *combination*, that the *freezing point* is much surpassed in the upper part of the *clouds*: hence the formation of grains of Sleet. *sleet* so *cold*, that in falling through the *clouds*, their size is increased in the form of *icicles*, by the watery vesiculæ freezing upon them; of which formation the hail-stones bear all the characters, especially by having in the centre that opaque grain of *sleet*.

Other phenomena observable in the atmosphere. The foregoing are the most conspicuous of the operations produced, at certain times, in some strata of the atmosphere, but not all those which an attentive observer may perceive: they are here, as must be the case in the first steps concerning all invisible processes producing visible effects, explained only by *general* analogies with known causes in our chemical processes; and if we cannot yet approach nearer to *specific* causes, it is because we are still very backward in the knowledge of the *subtile fluids*, which, at different times, come to mix with *air* in the atmosphere. We cannot however doubt, that to such *fluids* is owing the multitude of phenomena still unexplained intelligibly, both in the atmosphere itself, and in its connexions with *vegetation* and the animal economy, when we consider what progress has been made in this knowledge, by only attending to the *chemical affinities* of *light* and *fire*, and by a beginning of discovery on those of *electric fluid*, the existence of which on bodies would be unknown to us, were it not for the *motions* produced in visible bodies, by the disturbance of its *equilibrium*: this is one of its characteristic properties, and our test of the degrees of its intensity in different cases; as the *thermometer* is for *free fire*, and *vision* for *light*.

Tests of the actual state of These first steps in the knowledge of *causes*, which are themselves *imperceptible*, must render experimental philosophers

sophers more and more attentive to all the circumstances, that may lead to the discovery of new *tests* of the *actual* state of the *air*, in consequence of other *impalpable fluids* mixed with it; and also to the *electric* phenomena, that may appear in *chemical* processes, since the *electric fluid* is always present on the bodies which enter into *chemical* combinations, as it is present on all bodies: in this diffused state, it produces no known *chemical* effect; but all the phenomena before pointed out undoubtedly prove, that its *compositions* and *decompositions* have the greatest influence in the terrestrial phenomena. the air desirable.

The general character of the system above extracted from my works already published, by supposing a multitude of still unknown substances, will undoubtedly encounter the disapprobation of those philosophers, who consider *simplicity* as the characteristic of the operations of nature: but if this word has any sense, it must signify *enough* and *nothing more*; therefore, the first condition is *enough*, and when, in certain phenomena, we find a deficiency of known agents, the chasm is not to be filled up by arbitrary hypotheses, which are nothing; *analogy* is our only sure guide in the investigation of hidden *causes*, as being a thread offered to us by nature itself. Substances still unknown.

This is one of the precepts of the father of true philosophy, the immortal BACON, who taught us, in particular, not to dread the multitude of substances, when they are wanting for the production of phenomena accurately defined. Among his remarks on this subject is the following, under the 98th head of his *Silva Silvarum*; which remark I have the more admired, the longer I have studied the phenomena of our globe. "Cognitio humana determinata hactenus "fuit *speculatione* & *visu*; ita ut, quicquid oculos fugeret, "sive propter tenuitatem corporis, aut partes exiles, aut "subtilitatem motus, parum sit exploratum. Hæc tamen "naturam maxime regunt, illisque posthabitis, vera ana"lysis institui nequit, aut indica rinaturæ processus. *Spi*"*ritus* aut *pneumatica* (expansible fluids) quæ omnibus "tangibilibus insunt, vix cognoscuntur . . . *Spiritus* enim "nihil sunt præter *corpora naturalia*, proportionaliter *ra*"*refacta*, tangibilibus corporum partis inclusa velut tegu"mento: Doctrine of Bacon.

" mento ? neque minus *inter se* differunt, quam *densæ* &
" *tangibiles* partes, omnibusque *tangibilibus corporibus* in-
" sunt plus minusve, & plerumque nunquam cessant. Ab
" his, eorumque motibus, præcipue procedunt arefactio,
" colliquatio, concoctio, maturatio, putrefactio, vivificatio,
" & præcipua naturæ effecta *."

Occult causes. Not to admit the existence of such substances, because they escape our *sight* or *touch*, would be returning back to *occult properties*, *essential qualities*, which, in the infancy of natural philosophy gratified the imagination under the shape of *causes*. However, these conceptions were a beginning of knowledge, as under that form were gathered a certain number of important *phenomena*, successively observed; but of these the *agents* were still to be sought for.

Imponderable substances, However it has been only at the birth of *pneumatic physics*, and when its progress has occasioned the investigation of the *chemical* affinities of *light* and *fire*, that many mysteries in nature have been unfolded; and what a field of new researches has been opened by the attention given to a third *imponderable substance*, the *electric fluid !* Now, these very great steps teach us, that no progress, marked by such memorable epochs, and followed by so many important consequences, can be expected but by farther discoveries in

still to be discovered, the same class of substances, some of which may happen to manifest themselves also by characteristic effects, either known but mistaken, or yet unnoticed, and in these cases they might in some degree be submitted to analysis, by the changes they operate in certain phenomena, already known, but not sufficiently determined.

* " Human knowledge has hitherto been guided by viewing and beholding; so that whatever escapes our eyes, either from the smallness of the body itself, the tenuity of its particles, or the subtility of its motions, is but little explored. By these however nature is chiefly governed; and if they be neglected, a just analysis cannot be made, or the processes of nature disclosed. The expansible fluids, that exist in all tangible substances, are scarcely known. These fluids are nothing but natural bodies, proportionally rarefied, included in the parts of tangible substances as in a case: nor do they differ less from each other, than the dense and tangible parts, they inhere more or less in all tangible bodies, and for the most part are never still. To these, and their motions, are owing in particular rarefaction, dissolution, concoction, maturation, putrefaction, vivification, and the principal effects of nature." C.

It

It is not to be expected, that, by groping in a desultory manner among the objects of nature, any main road of investigation can be opened for the discovery of new *causes*; as their effects are so much intermixed in perceptible *phenomena*, that we cannot ascend to them with certainty in a retrograde manner. Many more discoveries concerning them may be expected from researches carried on by connected steps along the roads already opened in the maze of *imponderable* substances, the greatest agents in the phenomena of nature.

lead to a further knowledge of nature,

The modifications of the *sun's rays* to produce *heat*, as followed by Mr. de Saussure and Dr. Herschel, and I may say by myself; as well as the first observations made by Dr. Priestley on the *chemical* effects of *light*, have opened one of these roads, which requires to be pursued in all its ramifications. Much is to be done also concerning the nature of *fire*, i. e. the cause of *heat*, or of that *expansion* of bodies of which the *thermometer* is the measure; a road which has been much obstructed by the obscure idea of *caloric*, introduced in the modern theory of chemistry, at the time when several experimental philosophers were engaged in researches concerning the nature, modifications, and combinations of the *expansible fluid* long known under the name of *fire*. Much more remains to be done in the study of the *electric fluid*, its production and decomposition throughout so many phenomena. Lastly, almost every thing remains to be done to acquire some knowledge of a *fluid*, the existence of which is manifested by some characteristic effects, but which is itself totally unknown; though it cannot be without some, and it may be a great influence in terrestrial phenomena: I mean the *magnetic fluid*, on which I shall say here only a few words.

agreeably to what has already been effected.

The causes of heat,

electricity,

and magnetism.

Being now informed, that the *motions* of bodies occasioned by *amber* when it has undergone *friction*, of which the cause was unknown to the ancients, are the effects of a *fluid*, which has much greater functions in nature by its compositions and decompositions; when we come to consider the analogous, though much more limited effects produced by *steel bars* which have undergone proper *frictions*, we are led to conclude, not only that these particular *motions* are

Magnetism probably owing to a peculiar fluid, which has other effects.

are also the effects of a particular *fluid*, but that its functions in nature are not confined to those of attracting or repulsing *iron* according to circumstances, and producing in a movable *needle* the property of keeping more or less parallel to the meridian of the place, with a determined end pointing towards the north; though the latter, by its use in navigation, is become of great importance.

Its phenomena observed by Van Swinden.

With respect to this astonishing phenomenon, Prof. Van Swinden of *Franeker* has much advanced what Bacon calls the *History* in every class of phenomena, by an indefatigable perseverance in observing the variations of the *magnetic needle* compared with various circumstances. This, for every phenomenon, is the first step toward the discovery of *causes*; for the nature of those that may be devised must answer to all the modifications of the phenomena carefully observed, before confidence can be granted to any hypothesis.

Its polarity particularly to be studied.

In *magnetism*, the main point which must direct the natural philosopher in search of a cause is the same which directs the navigator, namely the *direction* of the *magnetic needle*; for this must belong to a cause, which in some manner influences the whole Earth. This consideration has suggested to Prof. Prevost of Geneva an idea, which, though not completed, deserves notice. After all the discoveries already made in meteorology and chemistry, it cannot be doubted, that *light* has, in various ways, a great share in the formation of many *atmospheric fluids*, and thus probably of the *magnetic*: but there must be some cause of the formation of a greater quantity of it on the northern than the southern hemisphere of the Earth, since the *magnetic needle* has a tendency to turn that way. I shall not enter into particulars on Mr. Prevost's hypothesis, and shall only mention its ground, in order to show, that this object may not be unattainable; it is the circumstance, that the sun remains about 8 days longer on the northern side of the ecliptic, than on the southern.

Light probably concerned in it.

Saussure's magnetometer.

With respect to phenomena which may indicate a formation of this *fluid*, Mr. de Saussure has invented a very important instrument, which he has called *magnetometer*; showing variations in the intensity of *attraction* of a *magnet*

in

in different parts of the day, and also in different days and seasons, as the *aërial electroscope* shows variations in the *electric state* of the air in the same circumstances. These two kinds of *variations*, therefore, deserve to be followed, comparatively with each other, and in their connexion with other atmospheric phenomena, as these observations may forward our knowledge respecting the *magnetic fluid*, which probably, as well as the *electric fluid*, by its *composition*, *decomposition*, and *combination* with other substances, has an influence in terrestrial phenomena.

Both magnetism and electricity naturally inherent in some bodies, and produced in others by friction.

The *loadstone* with respect to *magnetism*, and the *tourmalin* to *electricity*, are bodies which produce these phenomena from their own nature; but there is a method in our power to produce them by other bodies, namely *friction*: it is therefore very important, in either case, to discover in what manner *friction* acts to produce these effects. We have yet no hold in this pursuit with respect to *magnetic* phenomena, but some light may be reflected upon them by a determination of the manner in which *friction* produces *electric phenomena*. I have studied this subject with much attention, and I propose to relate, in another paper, some experiments of this kind, leading to the analysis of the *electric machine*, and demonstrating the errour of the idea of two *kinds* of *electricities*, or of two fluids acting in the *electric* phenomena.

Ashfield, near *Honiton*,
1st October, 1810.

II.

The results of some Experiments on the sonorous Properties of the Gasses, by Mr. F. KERBY *and Mr.* MERRICK, *jun., of Cirencester.*

To Mr. NICHOLSON.

SIR,

Experiments on the sonorous properties of the gasses.

CONSIDERING the facility of procuring most of the gasses, it is, probably, in consequence of the difficulty of employing them for the purpose, that so few experiments have

have been made on their sonorous properties. We have lately been occupied in making a few experiments on this subject, the results of which I send, in as concise a manner as possible, for insertion in your celebrated Journal, if you should think them of sufficient value. In some particulars they are very incomplete. We were prevented from determining the intensity of the sound by surrounding noises, and variable winds; but we purpose repeating and extending these experiments, at a more favourable opportunity.

Apparatus employed. Our apparatus consists of a small pair of double bellows, fixed vertically in a wooden frame, having a brass screw underneath it, to fit into the plate of an excellent, single barrelled airpump. A thermometer is fixed against one arm of the wooden frame, and a small flute pipe of an organ (open at the end) against the other. A groove is made through this arm to convey the wind from the bellows to the pipe. See the dotted line Pl. VII, fig. 1. The whole is covered by a glass receiver, 13 inches high, and 7 in diameter; and the bellows are put in motion by turning backwards and forwards a bent wire, that passes through a collar of leathers at the top of the receiver, and is attached to another wire projecting from one end of a lever which has its other extremity fastened to the feeder of the bellows. Fig. 2. represents the pasteboard lining of the folds of the bellows.

Mode of making the experiments. After 80 strokes of the piston, the pipe was inaudible: after 200 strokes, the gas was transferred into the receiver from a bladder in the usual way; and the bridge of the monochord was moved till the sound of the wire was perceived to be the octave below that of the pipe: then half the length of the vibrating part of the wire, in thousandths of the whole length, was set down in the fifth column of the following table. In the experiments *c*, *d*, *n*, *p*, *r*, (column 1) the gas was transferred in four nearly equal portions. The monochord was previously tuned by a *c* tuning fork, to Earl Stanhope's "first bass *c*"*, Professor Chladni's

* "Principles of the Science of Tuning," 1806.

at

Nicholson's Philos. Journal, Vol. XVIII. p. 270.

Apparatus for Shewing the Sonorous properties of Gasses.

Apophyllite

Fig. 3

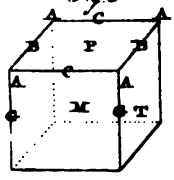


Fig. 5.

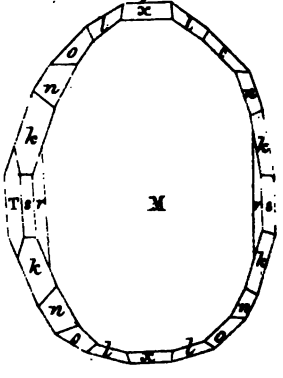


Fig. 4.

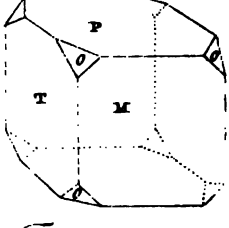


Fig. 1.

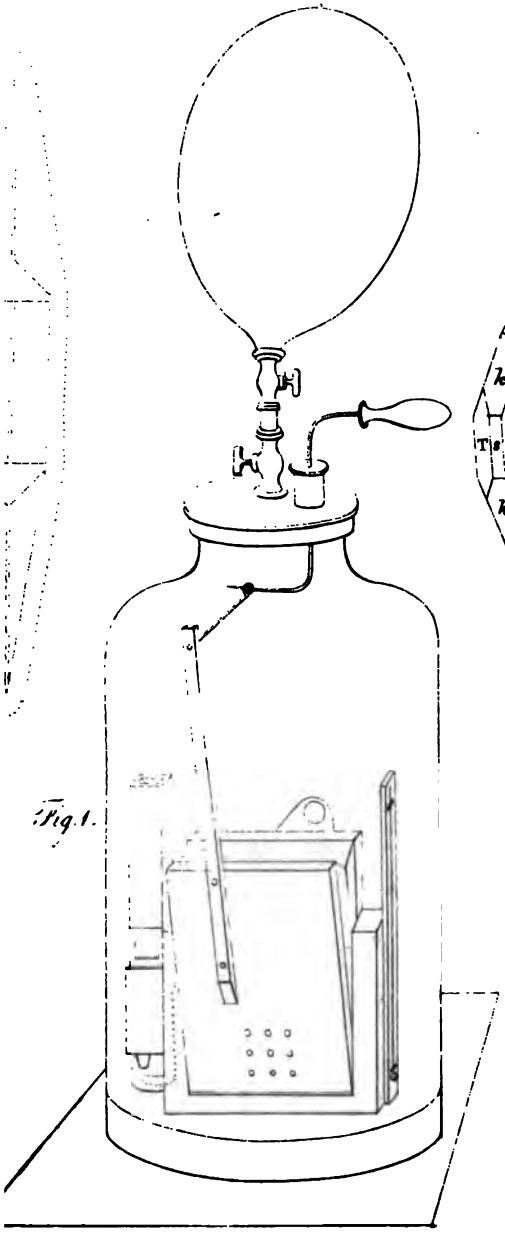
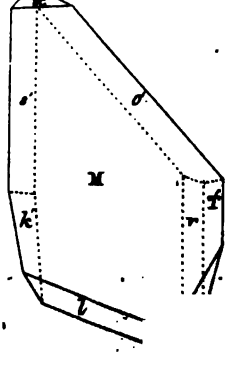
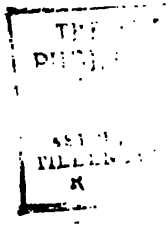


Fig. 6.





at 2†, or "*c* of the small octave," in the German notation‡.

I am, Sir,

Your obliged humble servant,

ARNOLD MERRICK.

Cirencester, October, 1810.

Tabulated results of the experiments.

Exper.	Barom	Therm	Aeriform fluids.	Mono-chord lengths.	Distan. heard. Feet.	Remarks.
a 1	29·69	57°.	Atmospheric air	·095	.....	At midnight.
b 1	——	——	Oxigen gas	·100	.....	From ox. of manganese.
c 1	—·68	60	Carbonic acid g.	·105	.....	1st Portion.
2	——	——	—— —— ——	·111	.....	2d.
3	——	——	—— —— ——	·112	.....	3d.
4	——	——	—— —— ——	·113	.....	4th.
d 1	——	61	Hydrogen gas	·053	.....	Obtained by means of zinc, &c.
2	——	——	—— —— ——	·052	.....	
3	——	——	—— —— ——	·049	.....	
4	——	——	—— —— ——		.....	
e 1	——	——	Atmospheric air	·093	.....	
f 1	——	——	Nitrous gas	·100	.....	From copper and nitric acid, &c.
2	——	——	—— —— ——	·083	.....	
3	——	——	—— —— ——	·083	.....	
g 1	—·56	——	Atmospheric air	·095	.....	
h 1	—·53	66	Carbonic acid g.	·117	310	From marble, &c.
2	——	——	—— —— ——	·115	342	
i 1	——	65	Atmospheric air	·095	273	
2	——	64	—— —— ——	·095	1236	The receiver taken off the apparatus.
k 1	—·47	——	Ether vapour	·065	57	Every new dose of ether made the tone lower in pitch, at first.
l 1	—·38	63	Oxigen gas	·099	.....	
2	—·37	——	—— —— ——	·098	245	Strong wind.
m 1	—·36	61	Atmospheric air	·094	245	
n 1	—·49	65	Hydrogen gas	·047	.....	Like the sound of a little bell.
2	——	——	—— —— ——	·044	.....	
3	——	66	—— —— ——	·044	.....	
4	——	——	—— —— ——	·042	146	
1	—·48	70	Nitrogen gas	·089	.....	Full, smooth, sound.
2	——	69	Hydrogen	·061	.....	
3	—·45	66	No addition	·072	.....	
4	——	——	Carbonic acid	·082	.....	
5	——	——	Oxigen	·083	.....	
6	——	——	Breath	·088	.....	
p 1	— 44	——	Light carbu. hyd.	·088	.....	Procured from wood.
2	——	——	—— —— ——		.....	
3	——	——	—— —— ——	·089	.....	
4	——	——	—— —— ——	·090	341	
q 1	——	——	Atmospheric air	.....	379	
r 1	——	——	Nitrous oxide	·108	.....	Obtained from the Nitrate of Ammonia
2	——	——	—— —— ——	·112	.....	
3	—·43	——	—— —— ——	·113	.....	
4	——	65	—— —— ——	·115	371	

† Chladni's Acoustics.

‡ Callcott's Musical Grammar, p. 16. 1809. Dr T. Young's Lect. 2. 568. 1807.

III.

Description of the Apophyllite, Ichthyophthalmite of Dandrada and Reuss, Fischaugenstein of Werner. By Mr. HAUY*.

Fish's-eye stone considered as a zeolite,

THE mineral, which is the subject of the present article, appears to have been anciently known, and was classed as a species of zeolite, from its property of forming a jelly with acids. It had been analysed by Rinman, who mentions it by the name of zeolite of Hellesta, in Sweden. The results of his analysis are nearly the same as have lately been obtained by my celebrated colleagues, Fourcroy and Vauquelin, and by Mr. Rose, whom Prussia has recently lost, to the great regret of every friend of science.

or a feldspar.

Mr. Dandrada's description of this stone does not appear to me to mark it by characters sufficiently precise, to allow us to decide, whether it should occupy a separate place in the system, or be classed with some of the known species. Mr. Brochant, after having quoted the principal features of this description, adds, that the ichthyophthalmite appears to have several of the characters of feldspar: and the name given it by Mr. Dandrada accords with this analogy, the name being equivalent to that of fish's eye, which in the language of the old French mineralogists was applied to that variety of feldspar, which I call pearly, and which is the moonstone of our lapidaries.

to a species of which the name was formerly applied:

but it is a distinct species.

On examining some specimens brought hither about three years ago by Mr. Molir, I was convinced, that the ichthyophthalmite is clearly distinguished by its mineralogical characters, not only from feldspar, but from all other known minerals. I shall therefore proceed to detail these, which are already given for the most part in the work, which Mr. Lucas, jun., had drawn up from my public lectures, under the title of *Tableau méthodique des Espèces minérales.*

Its characters.

Essential character. Divisible into a rectangular parall-

* Journal des Mines, vol. xxiii, p. 385.

elopipedon,

lopipedon, having a triple tendency to exfoliation, by fire, by acids, and by friction *.

Physical char. Specific gravity 2·467.

Hardness: not scratching glass, and giving no sparks with steel: scratching fluate of lime feebly, and carbonate of lime very evidently. If a fragment be rubbed sidewise on a hard substance, as if to polish it, it splits into leaves.

Refraction, simple.

Electricity, easily excited by friction. It is the vitreous.

Lustre. The surface of the crystals has a mean lustre between glassy and pearly, united with a transparency in general decided, without any proper colour.

Fracture, conchoidal, moderately shining.

Geometrical characters. Its primitive form, Pl. VII, fig. 3, is a quadrangular right prism with rectangular bases. The divisions parallel to M are very clear, and easily obtained. Those answering to P and T are not very evident except in a strong light †.

Chemical characters. Exposed to the flame of a candle, it splits into leaves. Before the blowpipe it fuses with difficulty into a white enamel. Immersed in cold nitric acid it divides in a few hours into small fragments, which at length become a white flocculent matter. Its powder forms in it a kind of jelly, similar to that produced under the same circumstances by the mesotype, or zeolite.

Analyses of the apophyllite.

	By Rinman,	Fourcroy and Vauquelin,	Rose.	
Silex	55	51	55	Component parts.
Lime	27	28	25	
Magnesia	0·5			
Potash		4	2·25	
Alumine	2·5			
Water	17	17	15	
	102	100	97·25	

* From this character I have taken the name of apophyllite, signifying, "a stone that exfoliates."

† The proportions of the three dimensions C, G, B, are those of the numbers $\sqrt{8}$, $\sqrt{9}$, $\sqrt{13}$.

Crystalline forms.

The specimens of apophyllite I have examined exhibit a few crystalline forms, among which the most simple is that seen on a groupe in the museum of natural history. It is the primitive parallelopipedon, the eight solid angles of which are cut off by triangular facets, *o*, *o*, *o*, fig. 4. The angle of incidence between *o* and M is 110° 50′.

Another variety, which I call supercompounded, is that represented fig. 5. The following are its principal angles of incidence. Between M and T, 90°: M and *s* 121° 57′: M and *r*, 149°: M and *k*, 118° 11′: M and *n*, 135° 32′: M and *o*, 110° 50′: M and *l*, 109° 32′: M and *x*, 119° 1′.

Very uncommon crystal.

The specimen, from which I determined this variety, is one of the most remarkable, that has come under my notice, since I began to study crystallography. It adhered in a single point only, as I may say, to its support, from which I separated it by a slight stroke. It followed from this position, that the crystal has its terminations on every side, which is itself not very common. But a still more uncommon circumstance is the contrast presented by all the parts similarly situate, when we compare them with each other. In general, when a crystal deviates from symmetry, it is only by the absence of a small number of facets, among those that are necessary to the integrity of the whole; so that these facets appear to have escaped the laws tending to produce them merely by accident, and the observer has little trouble to restore them in imagination. But in this crystal, which is represented exactly as it was formed fig. 6*, there is only one of the faces situate on one side, that has a corresponding face opposite to it: none of the other faces are repeated on the corresponding parts; and such is the progress of the decrements, that several of the faces which are single, as *o*, *n*, *k*, &c., ought to show themselves in eight different places, to leave no deficiency in the form of the crystal. It required time and study, to supply all these unexpressed circumstances of the crystallization, and reduce this sort of sketch, composed of ten faces seemingly without any connexion, to the real type of the form, which exhibits a well arranged assortment of

* The faces T′, *o*′, *r*′, *s*′, *k*′, belong to the back part of the crystal.

forty-

forty-eight faces. It will be easy to perceive the connexion of this type with the crystal that includes only its elements, from the identity of the letters marking the corresponding faces on the two polyedrons, fig. 5 and 6*.

Integrant particle.

Among the various forms of integrant particles, that are rectangled parallelopipedons, I know no one, that does not differ perceptibly from that of the apophyllite in the ratio of its dimensions, which alone is sufficient to show, that this mineral substance ought to be considered as a distinct species. On this subject I think it may not be amiss, to repeat what I have said elsewhere: it is not simply in the number and positions of the natural junctures, that the geometrical character consists, which I employ to distinguish one species from another, but also in the comparative dimensions of the forms of the particles. Hence arises a system of crystallization, which accords only with the substance possessing this form, unless it be a limit capable of belonging to several minerals, as the cube, regular tetraedron, &c.; in which case it is necessary to add an auxiliary physical character to that derived from the form of the particles, that the species may be determined unequivocally. The analyses I have mentioned tend equally to establish an essential distinction between the apophyllite and all other minerals, and thus the results of chemistry and mineralogy with respect to this substance fully satisfy the two conditions enunciated in my definition of a species, considering this as an assemblage of natural bodies, the integrant particles of which are similar in form, and composed of the same principles united in the same proportions. "Mineralogy will have attained perfection, when we find throughout that conformity between the operations of two sciences, which should continually assist each other; and the agreement of which, as they investigate nature by very different paths, must doubly confirm the truths they disclose."

Haüy's rule for distinguishing species.

* Fig. 6 represents the ten faces of the elementary crystal: fig. 5 shows only the twenty four faces of the complete crystal, supposed to be seen in front; but it is easy to conceive in imagination the other twenty four, which are on the back part.

IV.

On the Motion of Rockets both in Nonresisting and Resisting Mediums. By W. MOORE, *Esq.; communicated by the Author.*

To Mr. NICHOLSON.

SIR,

SHOULD the following Essay on the Motion &c. of Rockets be considered sufficiently interesting for your valuable and well conducted Journal, you are at liberty to make use of it.

I am, Sir,

Yours very respectfully,

W. MOORE.

Royal Military Academy, Woolwich,
November 3, 1810.

The theory of rockets not investigated by mathematicians.

The theory of rockets is a subject, which has never yet engaged the attention of mathematicians; a circumstance which perhaps is partly to be ascribed to their not having been used until very recently as implements of warfare. The practice however of throwing them into besieged places, to cause their surrender, is now nearly universal among the English, and indeed is almost confined to them.

Their military use.

The invention of the military rockets* (as they are now called) as it regards the exemption of our troops from the enemy's power of annoyance, is to be esteemed as valuable. By the help of these machines the capital of Denmark and the well fortified town of Flushing, together with much of the French navy have within a few years been taken and destroyed with scarcely the loss of a single man: on which account, it is a matter of no small moment to bring the rules for discharging them and the methods of estimating their effects under various circumstances into one general

* The invention of the military rocket is exclusively due to William Congreve, Esq.; a gentleman well known and esteemed by the public for his ingenuity.

and

and complete system; especially as their use is likely to become greater, and the improvements in making them extended.

Mode of their projection.

The manner in which the military rocket is projected is from a case or long hollow barrel, fixed strongly to a frame or carriage like the several pieces of cannon: the rocket being first screwed to a lath or rod of some inflexible substance, to prevent any irregularity or perturbation of motion in it during its flight: and the elevation of the machine, from which it is thrown in any required case of devastation, belongs, like that of artillery, to an experienced practitioner.

Measure of the strength of their composition.

It is a matter of no consideration to the rocket engineer, to know the proportion of the several ingredients, with which the rocket matter is made, provided the *measure* of the *strength* of the composition be given. Such important *datum* in this very interesting theory I have not at *present* been able, for want of experiments, to ascertain; but it is presumed, that the force of the fluid generated from firing it cannot differ very much from that from fired gunpowder, which is about 1000 times as great, (according to Robins,) as the pressure of the atmosphere; and until I am able to convince myself otherwise, I shall adopt this as the measure of the strength of the rocket composition.

Before entering upon the several computations respecting the motion of rockets; it will not perhaps be wholly ungratifying to readers in general, to exhibit two or three of these machines, and give some little description of them.

The rocket described.

A C D B (Plate VIII, fig. 3,) is the case of the rocket of cylindrical figure and made of sheet iron; *a* the place where the rocket is fired at the base of it A B; and C G D is the head of the rocket in the form of a right cone, and filled with inflammable matter, that consumes much more slowly than that with which the case or body is filled. This head is made also of sheet iron, and is quite solid near the apex G, in order that it may the better enter any object of penetrable substance, as ships of war, and all buildings composed of combustible and yielding materials. The white spots in the head denote holes, through which the fire and flame rush and fire the building into which the rocket penetrates.

The

The head is about nine inches in length; the length of the case A C being about 2¼ feet.

When the head of the rocket is not used, as in the case of firing it among troops; then the machine is simply in the form of that represented by *Fig.* 4; having about ¼ of a foot of its length from C to E filled with grape and canister shot; at the extremity of which. is a quantity of gunpowder E F *n m* to give them an additional impetus after the consuming of the wild fire; and by this means causing them to kill and terrify the enemy at a very great distance.

Investigation of their effects

This description of the military rocket being all that is necessary to my farther inquiries, I shall decline saying any thing more about it in this place, and proceed immediately to the investigation of their several effects; the nature and times of their motions; the angles at which they ought to be thrown, to fall upon an object at a given distance; and what ranges are within their sphere of devastation, &c. For all these are very important matters for military engineers and artillerists to know; to whom the management of them belongs, and whose object it therefore should be to prosecute such inquiries, and to render themselves masters of every particular, which the theory as well as the practice of throwing rockets embraces.

an object of importance.

Laws of their motion in a nonresisting medium.

On the Motion &c. of Rockets in a Nonresisting Medium.

Prop. I.

Prop. 1.

The strength or first force of the gas from the inflamed composition of a military rocket being given; as also the weight of the quantity of composition the rocket contains, together with the time of its burning, and the weight and dimensions of the rocket; to find the height it will ascend if projected perpendicularly, and also the velocity acquired at the end of that time; the laminæ of the composition being supposed to fire uniformly, and to burn parallel to the rocket's base.

Put w = weight of the case of the rocket and head
c = weight of the whole quantity of matter with which it is filled } D
q = time in which the same is consuming itself uniformly } P

p = 230 ozs

n = 230 ozs. the medium pressure of the atmosphere on 1 square inch

s = 1000 times the pressure of the atmosphere; or force of the inflamed composition

d = diameter of the rocket's base

x = PD the space the rocket describes in the time t, and

v = the acquired velocity in that time. Then, ed^2 is equal to the area of the rocket's base (e being ·7854 the area of a circle the diameter of which is 1) and ned^2 the pressure of the atmosphere on a surface $= ed^2$. Hence $sned^2$ is the constant impelling force of the composition.

Now the weight of the quantity of rocket matter that is fired or consumed in the time t is $\frac{ct}{a}$; therefore $c - \frac{ct}{a}$ is the weight of the part unconsumed; which added to w gives $w + c - \frac{ct}{a} = m - \frac{ct}{a}$ (by putting $m = w + c$) for the weight of the whole mass at the end of the time t; or when the rocket has ascended to D; and so far as weight resists the motion of the rocket, this muſt be deducted from the impelling force. Hence $sned^2 - \left(m - \frac{ct}{a}\right)$ is the *motive* force at D; and $\frac{sned^2 - \left(m - \frac{ct}{a}\right)}{m - \frac{ct}{a}} = \frac{asned^2}{am - ct} - 1$ the *accelerative* force.

By the theory of variable forces we have generally $\dot{v} = 2gf\dot{t}$ (where f denotes the accelerative force and $g = 16\frac{1}{12}$ ft). Therefore $\dot{v} = \frac{2agsned^2\dot{t}}{am - ct} - 2g\dot{t}$; the fluent of which is $v = -\frac{2agsned^2}{c} \times \text{hyp. log.} \left(\frac{am}{c} - t\right) - 2gt$.

Now when $t = o$, $v = o$; therefore the fluent corrected will be $v = \frac{2gasned^2}{c} \times \left(\text{hyp. log.} \frac{am}{c} - \text{hyp. log.} \frac{am - ct}{c}\right) - 2gt = \frac{2agsned^2}{c} \times \text{hyp. log.} \frac{am}{am - ct} - 2gt$; which,

when

when t becomes a, is $v = \frac{2agsned^2}{c} \times$ hyp. log. $\frac{m}{m-c} - 2ag$; or, because $m = w + c$, it will be $v = \frac{2agsned^2}{c} \times$ hyp. log. $\frac{w+c}{w} - 2ag$; which therefore is the velocity of the rocket when all the matter of inflammability in its body is just consumed.

This exemplified in numbers.

For an example in numbers, suppose the weight, dimensions, &c. to be as below; namely,

$s = 1000$
$n = 230$ ozs.
$w = 18$ lbs. $= 288$ ozs.
$c = 10$ lbs. $= 160$ ozs.
$a = 3$ sec.
$d = 3$ in. $= \frac{1}{4}$ ft.
$g = 16$ ft.
$e = .7854$

Then the above expression for v, namely $\frac{2agsned^2}{c} \times$ hyp. log. $\frac{w+c}{w} - 2ag = \frac{2 \times 3 \times 16 \times 1000 \times 230 \times .7854 \times \frac{1}{16}}{160} \times$ hyp. log. $\frac{448}{288} - 96 = 6774.075 \times$ hyp. log. $\frac{14}{9} - 96 = 2992.9895 - 96 = 2896.9895$ feet, the velocity of the rocket per second at the instant of exhaustion of the wildfire.

To find the space x, we have by the doctrine of variable forces $\dot{x} = v\dot{t} = b\dot{t} \times$ hyp. log. $\frac{am}{am - ct} - 2gt\dot{t}$ (where b represents the fraction $\frac{2agsned^2}{c}$).

Method of finding the fluent for the time of the ascent of the rocket.

Now to find the fluent of this equation, we must first determine the log. of $\frac{am}{am-ct}$; which is done by first putting it into fluxions, and then finding its fluent in a series. Thus, the fluxion of the log. $\frac{am}{am-ct}$ being $\frac{c\dot{t}}{am-ct}$, we shall

by

by expanding the fraction and taking the fluent of each term have, for the log. $\frac{a\,m}{a\,m - c\,t}$ the series $\frac{c}{a\,m} \times \left(t + \frac{c\,t^2}{2\,a\,m} + \frac{c^2 t^3}{3\,a^2 m^2} + \frac{c^3 t^4}{4\,a^3 m^3} + \frac{c^4 t^5}{5\,a^4 m^4} + \&c\right)$. Hence the above fluxional expression becomes $\dot{x} = \frac{b\,c}{a\,m} \times \left(t\,\dot{t}' + \frac{c\,t^2 \dot{t}'}{2\,a\,m} + \frac{c^2 t^3 \dot{t}'}{3\,a^2 m^2} + \frac{c^3 t^4 \dot{t}'}{4\,a^3 m^3} + \frac{c^4 t^5 \dot{t}'}{5\,a^4 m^4} + \&c\right) - 2\,g\,t\,\dot{t}'$; whose fluent is $x = \frac{b\,c}{a\,m} \times \left(\frac{t^2}{2} + \frac{c\,t^3}{6\,a\,m} + \frac{c^2 t^4}{12\,a^2 m^2} + \frac{c^3 t^5}{20\,a^3 m^3} + \frac{c^4 t^6}{30\,a^4 m^4} + \&c\right) - g\,t^2$, which wants no correction: therefore in the case where $t = a$; $x = \frac{b\,c}{a\,m} \times \left(\frac{a^2}{2} + \frac{c\,a^2}{6\,m} + \frac{c^2 a^2}{12\,m^2} + \frac{c^3 a^2}{20\,m^3} + \frac{c^4 a^2}{30\,m^4} + \&c\right) - a^2 g = \frac{a\,b\,c}{2\,m} \times \left(1 + \frac{c}{3\,m} + \frac{c^2}{6\,m^2} + \frac{c^3}{10\,m^3} + \frac{c^4}{15\,m^4} + \&c\right) - a^2 g$; the space through which the rocket ascends during the time of its burning.

Hence retaining the numbers in the example above for the velocity, we shall have $x = \frac{6774{\cdot}075 \times 3 \times 160}{2 \times 448} \times \left(1 + \frac{160}{3 \times 448} + \frac{160^2}{6 \times 448^2} + \frac{160^3}{10 \times 448^3} + \frac{160^4}{15 \times 448^4} + \&c\right) - 144 = 362{\cdot}8{\cdot}96875 \times 1{\cdot}14622279$ (the sum of the series to 6 terms) $- 144 = 4159{\cdot}606684 - 144 = 4015{\cdot}606684$ feet, the space the rocket ascends through during the 3 seconds it is on fire.

Another method of determining the fluent.

The fluent of $b\,\dot{t}$. hyp. log. $\frac{a\,m}{a\,m - c\,t} - g\,t\,\dot{t}$, might indeed have been obtained without a series; for $b\,\dot{t}$. hyp. log. $\frac{a\,m}{a\,m - c\,t} = b\,\dot{t}$. hyp. log. $a\,m - b\,\dot{t}$. hyp. log. $(a\,m - c\,t)$ the fluent of the former part of which is evidently $b\,t$. hyp. log. $a\,m$; and the fluent of $\dot{t}$. hyp. log. $(a\,m - c\,t) = t$,

$= t$. hyp. log. $(a m - c t) +$ fluent of $\frac{c t t'}{a m - c t} = t$. hyp. log. $(a m - c t) - t - \frac{a m}{c}$. hyp. log. $(a m - c t) = \left(t. - \frac{a m}{c.} \right)$. hyp. log. $(a m - c t) - t = - \frac{1}{c} (a m - c t)$. hyp. log. $(a m - c t) - t$. So that the whole fluent will be $x = b t$. hyp. log. $a m + \frac{b}{c} (a m - c t)$. hyp. log. $(a m - c t) + b t - g t^2$ which when $x = o$, and $t = o$ is $\frac{b a m}{c}$. hyp. log. $a m$. Hence the fluent corrected is $x = \left(b t - \frac{b a m}{c} \right)$ hyp. log. $a m + \frac{b}{c} (a m - c t)$. hyp. log. $(a m - c t) + b t - g t^2$, and in the case where $t = a$ it is $x = \left(\frac{a b c - a b m}{c} \right)$ hyp. log. $a m + \frac{a b}{c} (m - c)$. hyp. log. $(a m - a c) + a b - a^2 g = (c - m)$. hyp. log. $a m + (m - c)$. hyp. log. $(a m - a c) + c - \frac{a c g}{b} = \frac{a b}{c} \left((m - c). \left(\text{hyp. log. } (a m - a c) - \text{hyp. log. } a m \right) + c - \frac{a c g}{b} \right) = \frac{a b}{c} \times \left((m - c). \text{hyp. log. } \frac{m - c}{m} + c - \frac{a c g}{b} \right)$.

This in numbers is $= 127{\cdot}0139 \times (288 + \text{hyp. log. } \frac{9}{10} \times 160 - 1{\cdot}133734) = 4015{\cdot}9827734$. So that it appears that by summing the foregoing series only to 6 terms gives the result within ·376089 part of a unit, of this method.

Height to which the rocket would ascend if the retardation from the force of gravity were constant.

Since we have found the velocity at the end of this space to be 2896·9195 feet per second; we shall, on the supposition that the retardive force of gravity remains constant from D have by the theory of uniform forces $\frac{v^2}{4gf} = \frac{\overline{2896{\cdot}9895}^2}{64 \times {\cdot}9993709} =$ 131261·131 feet for the height to which the rocket will farther ascend; which being added to that just determined 4015·9827735 ft. gives 135277·1137735 feet, for the whole height of the rocket above the surface of the Earth when it has

has just lost all its motion, which is nearly equal to 27 miles.

But if the height to which it will farther rise be demanded on the true principle, that gravity varies inversely as the square of the distance from the Earth's centre; Then, Height of the real ascent according to the true law of this retardation.

Putting r = C L the rad. of the Earth
a = C D the diftance of the point to which the rocket has already ascended from the centre C
x = C I any variable distance from C
v = velocity at I
and c = velocity at D = 2896·9895 ft.

Then $x^2 : r^2 :: 1 : \frac{r^2}{x^2}$ the retardive force of gravity at I when that of the surface L is considered as unity.

Hence $-v\dot{v} = 2gf\dot{x} = \frac{2gr\dot{x}}{x^2}$ (the negative sign being used because the velocity decreases) whose fluent is $v^2 = \frac{4gr^2}{x}$, which, when $x = a$, and $v = c$, is $c^2 = \frac{4gr^2}{a}$; therefore the fluent corrected will be $v^2 = c^2 + \frac{4gr^2(a-x)}{ax}$: So that when $v = o$, we shall have $c^2 + \frac{4gr^2(a-x)}{ax} = o$, and $x = \frac{4agr^2}{4gr^2 - ac^2}$ = (taking the Earth's radius at 3979 miles) 21145143·65521 feet, the whole height of the rocket from the centre of the Earth; and consequently 21145143·65521 $- r$ = 136023·65521 feet is the whole height from the surface. Whence also the height to which the rocket rises from the point where the impelling force of the composition ceases or is destroyed is 132007·67221 feet.

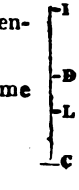
Hence it appears, that, in consequence of the diminution of the force of retardation from gravity upwards according to the inverse square of the distance from the Earth's

Earth's centre, the rocket will ascend nearly 746.54121 feet higher from a point 4230·609 feet above the Earth's surface with a velocity of 2896·9895 feet per second, than it would do if the same force as at the point D had continued constant, or had continued to act upon the body always with the same intensity. Hence also, if the rocket had a velocity of 2896·9895 feet per second upwards when at a height from the Earth's surface $= \frac{4gr^2}{c^2} - r$, it would never return; but continue to move for ever, or fly off to an infinite diſtance. For the expreſſion for x is $\frac{4agr^2}{4gr^2 - ac^2}$, or $x = \frac{4agr^2}{4gr^2 - ac^2}$; where it is evident that on ac^2 becoming $= 4gr^2$, x will be infinite; and therefore to find a, we have only to put $4gr^2 - ac^2 = o$ and reduce the equation.

Force that would prevent its returning.

Whence, having the height from which the body muſt fall to acquire a velocity, which, being added to that of 2896·9895 feet per second, ſhall cauſe it to move perpetually when projected with the velocity of their sum; we can readily determine what that velocity is; and it being a very curious fact to know, we will therefore give a solution to the problem in this place.

Put $d = \frac{4gr^2}{c^2} =$ C I the given height from the centre C
$x =$ C D, any variable height from the same point greater than the rad. C L
$r =$ C L



Then $\frac{r^2}{x^2}$ is the accelerative force of gravity at D when that at the surface is 1. Therefore $v\dot{v} = -2gf\dot{x}$; and the fluent of the same is $v^2 = \frac{4gr^2}{x}$; which when properly corrected is

$$v^2 = 4gr^2\left(\frac{1}{x} - \frac{1}{d}\right) = (\text{when } x = r)\ 4gr^2 \times \left(\frac{1}{r} - \frac{1}{d}\right)$$
$$= 4gr^2$$

$= 4gr^2\left(\frac{d-r}{dr}\right) = \left(\text{because } d = \frac{4gr^2}{c^2}\right) 4gr^2 \times \frac{4gr-c^2}{4gr^2} = 4gr - c^2$. Therefore the velocity acquired in descending through $d-r$ is $v = \sqrt{4gr-c} = 36553{\cdot}3482$ feet per second; which, added to the given velocity 2896·9482 feet per second, gives 39450·2377 feet, or 7·471768 miles for the velocity of projection to cause a body to move to an infinite distance.

To be continued.

V.

Remarks on a new Principle introduced by LEGENDRE *in his Elements of Geometry. In a Letter from* THOMAS KNIGHT, *Esq.*

To Mr. NICHOLSON.

SIR,

MR. LEGENDRE in his "*Eléments de Géométrie*" (p. 311, 3d Ed.) has attempted to demonstrate certain propositions, by a new and very peculiar kind of reasoning; founded on the consideration of functions, and the homogeneity of quantities.

New mode of reasoning by Legendre,

The principle, introduced by this eminent geometer, appears to have been favourably received by his own countrymen; but has of late been alternately praised and censured by some of our writers: though no very convincing arguments have been advanced on either side of the question.

favourably received in France, and by some in Britain,

If you can afford me a place in your valuable Journal, I will endeavour to prove the fallacy of Mr. Legendre's reasoning; first, by showing, that it would lead to the most absurd conclusions; and, secondly, by clearly pointing out the errour in this mode of investigation.

but it is fallacious.

The

Proof of this.

The principle itself, as well as the small reliance that can be placed on it, may be understood from the following

*Theorem**.

If two sides of one triangle are equal respectively to two sides of another, the third sides also are equal.

For let A and B be two sides of a triangle, p the included angle, C the opposite side. If A, B, and p be given, C will evidently be completely determined. C therefore is a function of A, B, and p. But it is plain, that p cannot enter into this function: for let some line, as D, be represented by unity: then A, B, and C are numbers, and if there could be an equation between A, B, C, and p, we might find p in terms of A, B, C; whence p would be a number, which is absurd. It follows from this, that C is a function of A and B only; whence the truth of the proposition is manifest. Q. E. D.

It is needless to say, that the principle must be erroneous, which leads to *such* a conclusion.

Things that cannot be compared not therefore independent of each other.

A writer in the *Edinburgh Review*† asserts, that this reasoning "takes for granted nothing, but that an angle and a "line are magnitudes, which admit of no comparison." It is a sufficient answer to this; that the quantity of grain in our barns, and the weather which preceded the collecting it there, are quantities which admit of no comparison; and yet the former has a pretty evident dependance on the latter.

Use of the word function.

It may be observed in the above proposition, that the term *function* is used in a very restricted sense: merely denoting numerical equality, or at most equality of homogeneous quantities; whereas every one knows, that a quantity may be a function of (or dependant on) another, without any such *absolute* equality as is there supposed.

Fallacy of Legendre's reasoning.

It is of no use therefore, to have shown, that there can be no equation, *properly so called*, as $C = \varphi(A, B, p)$ between A, B, C, p, unless it could farther be proved, that there can be no analogy as $C \propto \varphi(A, B, p)$ between the same quantities. This may be very simply exemplified;

* This differs from the first of Mr. Legendre's only in this; that I have changed angles into sides, and the side into an angle.

† No. 29, p. 4.

In

In the sector of a circle, the angle at the centre is a function of the arc and the radius; viz: Angle $\propto \frac{\text{Arc}}{\text{Radius}}$; but this is no equation, except we make an arbitrary choice of units.

Does not almost the whole of dynamics consist of equations (or to speak more properly analogies) between *heterogeneous* quantitities? But I imagine the falsehood of this kind of reasoning is already sufficiently proved.

I am, Sir,

Your most obedient servant,

THOMAS KNIGHT.

Papcastle, Nov. 3d, 1810.

VI.

Description of an Hygrometer for Gasses, and the Method of Using it, to subject different Substances to their Action: by Mr. GUYTON-MORVEAU*.

SINCE philosophers have endeavoured to investigate the properties of aeriform fluids, various kinds of apparatus have been invented, for placing them in contact with substances, that by their action on them might elucidate their nature, or might form with them new combinations. Of late they have particularly felt the necessity of reducing gasses to the greatest dryness, that the phenomena produced by their essential constituent parts might not be confounded with those, that might result from the decomposition of the water given out by them. Ascertaining the dryness of gasses important.

The instrument, which I now lay before the class, appears to me well adapted to both these objects. It was not till I had several times tried it, that I resolved to have one finished with great care, and to communicate a description The instrument repeatedly tried.

* Annal. de Chim. vol. LXVIII, p. 5. The instrument here described was laid before the physical and mathematical class of the Institute on the 8th of August, 1808.

of

of it to those, who are aware that in these delicate experiments we have never too many means of securing ourselves against the errours of manipulation.

Description of it.

This apparatus being intended to be introduced, very accurately closed, under a receiver the mouth of which is immersed in mercury, it must be mounted wholly in iron. When the vessel is carried above the level of the mercury, it is easy to open it, to give an opportunity for the mutual action of the substance it contains and the gas under the receiver; which is effected by moving the dependent part of the counterpoise, previously disengaging the catch. To keep it in this position, nothing is required but a simple wooden bracket, with a notch cut in it to receive the handle of the instrument, in which it is fixed by a wedge. After having allowed it to stay as long as is necessary for the action required to take place, the glass vessel may be closed by its glass stopple, and taken out of the trough, without danger of any thing escaping from it, or of a single particle of mercury getting in, by means of the same catch, which is placed in the middle of the counterpoise, and strongly presses down the cover.

Method of using it.

To examine the hygrometrical state of any gas, take the glass bottle out of its collar, weigh it accurately, and fill it with dry pulverized muriate of lime, that has been in fusion; which likewise must be weighed. Having replaced it, and shut down the cover close, introduce it under the receiver, and then raise the cover. The weight gained by the muriate of lime will indicate the quantity of water absorbed.

Explanation of the figures.

Explanation of the plate.

Pl. VIII, fig. 1, represents the apparatus introduced under the receiver, the glass stopple raised by the hinged cover, to which it is cemented. The cover is kept up by the catch *g*; which in this situation is placed under the cross part of the handle. This figure is on a scale of two lines to an inch, English measure.

In fig. 2 the essential parts of the instrument are shown on a scale of four lines to an inch.

A is a glass bottle, holding two or three centilitres, the mouth of which is ground perfectly even; and confined by the screw *d* in the collar *B*, which opens with a joint at *C*,

so

Guyton - Morveau's Hygrometer for Gases.

Fig. 1.

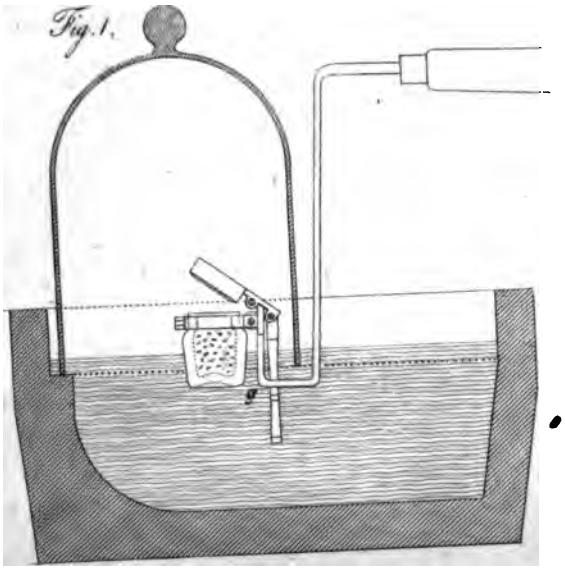
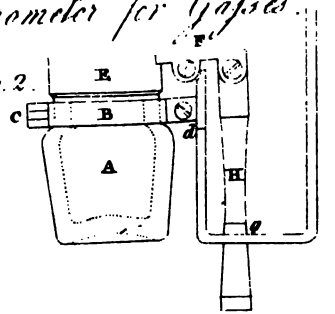


Fig. 2.



Military Rockets

Fig. 3.

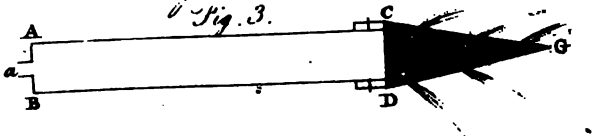
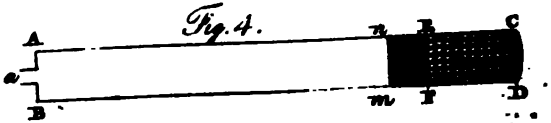
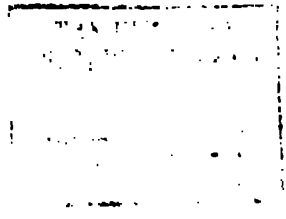


Fig. 4.





so that the vessel may easily be taken out, to be cleaned or weighed.

E the cover, to which the glass stopple is cemented. It is here seen closed by the position given to the counterpoise *F*, which is secured by the pressure of the catch *g*, in the movable part of the counterpoise *H*, on the elbow in the handle.

VII.

The Croonian Lecture. *By* WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S.**

Croonian lecture.

I AM aware that the remarks, which I have to offer on the present occasion, may be thought to bear too little direct relation to each other for insertion in the same lecture; yet any observation respecting the mode of action of voluntary muscles, and every inquiry into the causes which derange, and into the means of assisting the action of the heart and blood vessels, must be allowed to promote the design of Dr. Croone, who instituted these annual disquisitions. And it has always appeared to be one great advantage attending the labours of this society, that it favours the production of any original knowledge, however small, in a detached form; and enables a writer to say all that he knows upon a particular subject, without inducing him to aim at the importance of a long dissertation.

One of the advantages of the society.

I shall therefore make no apology for dividing the following lecture into three distinct parts.

Subjects of this paper.

In the first of which I shall treat of the duration of voluntary action.

In the second, I shall attempt to investigate the origin of seasickness, as arising from a simple mechanical cause deranging the circulation of the blood.

In the third, I shall endeavour to explain the advantage derived from riding and other modes of gestation, in assist-

* Philos. Trans. for 1810, p 1.

cording to the degree of force exerted in the experiment*.

As a farther proof, that I was not much deceived in my judgment of the frequency of these vibrations, I requested two or three of my friends to repeat the same experiment for me, and our agreement was such, as to confirm me in the opinion, that there could be no very considerable errour in the estimate.

Extremes of frequency.

The greatest frequency, that I think I have observed, was about 35 or 36 in a second, and the least was as low as 14 or 15; but in attempting to lessen the number of vibrations, there appears to be a degree of unsteadiness, which prevents any accurate measurement of the real number.

Trembling of age or weakness.

It is very probable, that in cases of great debility the number may be even considerably less, and may be the reason of that visible unsteadiness, which is known to occur in persons enfeebled by age, or much reduced by disease.

The observation not new,

Possibly the foregoing observation may not be altogether new to some members of this Society, as it is now about 17 or 18 years since it first occurred to me, and I was then accustomed occasionally to mention it in conversation with my friends; but I am not aware that any other person has made

but the fact ascribed to a wrong cause.

the same remark respecting the vibratory nature of muscular action, although I find, that Grimaldi had observed the sound that occurs upon stopping the ears, but ascribed it, according to the notions that prevailed in his time, to the hurried motions of the animal spirits†.

Sound of carriages at a distance.

* The resemblance of the muscular vibrations to the sound of carriages at a distance I apprehend to arise not so much from the quality of the sound, as from an agreement in frequency with an average of the tremors usually produced by the number of stones in the regular pavement of London, passed over by carriages moving quickly.

If the number of vibrations be supposed 24 in a second, and the breadth of each stone be about 6 inches, the rate of a carriage thus estimated would be about 8 miles an hour, which agrees with the truth as nearly as the assumptions on which the estimate is founded.

† Vera itaque ratio experimenti prædicti est, quia in digito et brachio totoque corpore continuato fiunt multi motus ac tremores, ob spirituum agitationem huc illuc perpetuo accurrentium.

GRIMALDI, Physicomathesis de Lumine, p. 383.

PART II. *On Seasickness.*

Seasickness.

The second remark which I have to offer to the society relates to seasickness, the cause of which has not hitherto been fully explained; and although the explanation which I am about to propose may not appear altogether satisfactory to persons, who, when at sea, are also rendered giddy by the incessant motion of the waves, and are consequently liable to consider as cause and effect phenomena which in their minds are constantly associated, yet the observation on which it is founded may deserve to be recorded, on account of the degree of relief that may be obtained in that most distressing affection.

Power of resisting its effects acquired.

After I had been harassed by seasickness during a short voyage for some days, and had in vain attempted to account for the difference between the inexperienced passenger, and those around him more accustomed to the motion of the sea, I imperceptibly acquired some power of resisting its effects, and had the good fortune to observe a peculiarity in my mode of respiration, evidently connected with the motion of the vessel, but of which, in my then enfeebled state, I was unable to investigate either the cause or consequence. In waking from a state of very disturbed sleep, I found that my respirations were not taken with the accustomed uniformity, but were interrupted by irregular pauses, with an appearance of watching for some favourable opportunity for making the succeeding effort; and it seemed as if the act of inspiration were in some manner to be guided by the tendency of the vessel to pitch with an uneasy motion.

Respiration affected by the motion of a vessel;

which influences the circulation of the blood.

The mode by which I afterward conceived, that this action could primarily affect the system, was by its influence on the motion of the blood; for, at the same instant that the chest is dilated for the reception of air, its vessels become also more open to the reception of the blood, so that the return of blood from the head is more free than at any other period of a complete respiration. On the contrary, by the act of expelling air from the lungs, the ingress of blood is so far obstructed, that, when the surface of the brain is exposed by the trepan, a successive turgescence and subsidence of the brain is seen in alternate motion with the different

Headach.

different states of the chest. It is probably from this cause, that, in severe headachs, a degree of temporary relief is obtained by occasional complete inspirations.

The respiration should counteract the pressure of the blood on the brain,

In seasickness also the act of inspiration will have some tendency to relieve, if regulated so as to counteract any temporary pressure of blood upon the brain; but the cause of such pressure requires first to be investigated.

which is produced by the subsidence of the vessel.

All those who have ever suffered from seasickness (without being giddy) will agree, that the principal uneasiness is felt during the subsidence of the vessel by the sinking of the wave on which it rests. It is during this subsidence, that the blood has a tendency to press with unusual force upon the brain.

If a person be supposed standing erect upon deck, it is evident that the brain, which is uppermost, then sustains no pressure from the mere weight of the blood, and that the vessels of the feet and lower parts of the body must contract, with a force sufficient to resist the pressure of a column of blood of between five and six feet from the head downwards.

If the deck were by any means suddenly and entirely removed, the blood would be no longer supported by its vessels; but both would fall together with the same velocity by the free action of gravity; and the same contraction of the vessels which before supported the weight of the blood would now occasion it to press upon the brain, with a force proportional to its former altitude.

In the same manner, and for the same reason, during a more gradual subsidence of the deck, and partial removal of support, there must be a partial diminution of the pressure of the blood upon its vessels, and consequently, a partial reaction upon the brain, which would be directly counteracted by a full inspiration.

The consequence of external motion upon the blood will be best elucidated by what may be seen to occur in a column of mercury similarly circumstanced.

The fact illustrated.

A barometer, when carried out to sea in a calm, rests at the same height at which it would stand on shore; but, when the ship falls by subsidence of the wave, the mercury is seen apparently to rise in the tube that contains it, because

cause a portion of its gravity is then employed in occasioning its descent along with the vessel; and, accordingly, if it were confined in a tube closed at bottom, it would no longer press with its whole weight upon the lower end. In the same manner, and for the same reason, the blood no longer presses downwards with its whole weight, and will be driven upwards, by the elasticity which before was merely sufficient to support it.

Sickness from swinging.

The sickness occasioned by swinging is evidently from the same causes as seasickness; and that direction of the motion, which occasions the most piercing sensation of uneasiness, is conformable to the explanation above given.

It is in descending forward, that this sensation is perceived; for then the blood has the greatest tendency to move from the feet toward the head, since the line adjoining them is in the direction of the motion. But when, in the descent backwards, the motion is transverse to the line of the body, it occasions little comparative inconvenience, because the tendency to propel the blood toward the head is then inconsiderable.

Not fully prevented by inspiration.

The regularity of the motion in swinging afforded me an apparently favourable opportunity for trying the effect of inspiration; but although the advantage was manifest, I must confess, it did not fully equal the expectations I had formed from my experience at sea. It is possible, that the suddenness of the descent may in this case be too great, to be fully counteracted by such means; but I am inclined to think, that the contents of the intestines are also affected by the same cause as the blood; and if these have any direct disposition to regurgitate, this consequence will be in no degree counteracted by the process of respiration.

Contents of the intestines affected also.

Effect on the stomach in some measure counteracted.

A friend of mine informed me, that he had endeavoured to counteract this mechanical effect upon the stomach, and had experienced immediate relief from a slight degree of seasickness, by lying down upon the deck with his head towards the stem of the vessel; by means of which, upon pitching, he was in the attitude of a person descending backwards in a swing.

Affect on the stomach instantaneous.

Whether the stomach be or be not thus primarily affected, or only by sympathy with the brain, the sensation of sinking is

is in all cases referred directly to the stomach, which is seized with such instantaneous retching, that no person who has not been so situate, can form a just conception of it*.

Tendency to faint from the pressure on the brain being withdrawn.

In thus referring the sensations of seasickness, in so great a degree, to the agency of mere mechanical pressure, I feel confirmed by considering the consequence of an opposite motion, which, by too quickly withdrawing blood from the head, occasions a tendency to faint, or that approach to fainting, which amounts to a momentary giddiness with diminution of muscular power. At a time when I was much fatigued by exercise, I had occasion to run to some distance, and seat myself under a low wall for shelter from a very heavy shower. In arising suddenly from this position I was attacked with such a degree of giddiness, that I involuntarily dropped into my former posture, and was instantaneously relieved, by return of blood to the head, from every sensation of uneasiness.

Since that time, the same affection has frequently occurred to me in slighter degrees, and I have observed, that it has always been under similar circumstances of rising suddenly from an inclined position, after some degree of previous fatigue. Sinking down again immediately removes the giddiness; and then, by rising a second time more gradually, the same sensation is avoided.

Earthquakes affect the stomach.

* There is one occasion, upon which a slighter sensation of this kind is perceived, and it appears to indicate the direction of the motion from which it arises, to be downwards. "In a country subject to frequent returns of earthquakes," it is said * "a few minutes before any shock came, many people could foretel it by an alteration in their stomachs; an effect which" (it is added) "always accompanies the wavelike motion of earthquakes, when it is so weak as to be uncertainly distinguishable." (Michell, Phil. Trans. vol. LI, 610.)

It seems, that the vapours to which these tremendous concussions are owing, immense in quantity, and of prodigious force, being for a time confined on all sides, elevate the surface of a country to a vast extent, until they either find vent, or meet with some partial cause of condensation; and hence the alternate heaving and subsidence of the ground will produce much the same effects as the rising and falling of the swell at sea.

* Phil. Trans. vol. XLII, p. 41.

PART III. *On the salutary Effects of Riding, and other Modes of Gestation.*

In the preceding instances of disturbing the circulation of the blood by external motion, the effect is disagreeable, and proportionally prejudicial. There may indeed be cases of disorder, in which it will be salutary; but these are probably less frequent, than is generally supposed. Gestation

In the observations which follow, general opinion will concur with me, on the benefit derived from external or passive motion; and I hope, that, in ascribing its good effects to their true cause, I shall enable others to make a valuable distinction, which has not yet been preserved with due care, between one motion which is salutary, and another which is very frequently pernicious. For, although the term *gestation* is employed by medical writers, as a general term comprehending riding on horseback, or in a carriage; and although the merits of such motions, especially the former, were clearly noticed, and perhaps even overrated, by the discernment of Sydenham; I believe, that no explanation has yet been given of the peculiar advantages of external motion, and am persuaded, that the benefits to be derived from carriage exercise are by no means in so high estimation as they ought to be. should be distinguished from exercise in general;

Under the common term *exercise*, active exertion has too frequently been confounded with passive gestation, and fatiguing efforts have consequently been substituted for motions that are agreeable, and even directly invigorating, when duly adapted to the strength of the invalide, and the peculiar nature of his indisposition. active being injurious, where passive is salutary.

The explanation, which I am about to offer, of the effects of external motion upon the circulation of the blood, is founded upon a part of the structure observable in the venous system, the mechanical tendency of which cannot be doubted. The valves, which are every where dispersed through those vessels, allow free passage to the blood, when propelled forward, by any motion that assists its progress; but they oppose an immediate obstacle to such as have a contrary tendency. The circulation is consequently helped forward by every degree of gentle agitation. The heart is supported Effects of gestation on the circulation of the blood,

supported in any laborious effort, that may have become necessary by some obstacle to its exertions; it is assisted in the great work of restoring a system, which has recently struggled with some violent attack; or it is allowed, as it were, to rest from a labour, to which it is unequal, when the powers of life are nearly exhausted by any lingering disorder.

other vital functions,

In the relief thus afforded to an organ so essential to life, all other vital functions must necessarily participate; and the various offices of secretion, and assimilation, by whatever means they are performed, will not fail to be promoted during such comparative repose from laborious exertion.

and even the mind.

Even the powers of the mind itself, though apparently least likely to be influenced by mere mechanical means, are manifestly, and in many persons, affected most immediately by these kinds of motion.

Inability from fullness of blood.

It is not only in cases of absolute deficiency of power to carry on the customary circulation, that the beneficial effects of gestation are felt, but equally so, when comparative inability arises from redundancy of matter to be propelled. When from fulness of blood the circulation is obstructed, the whole system labours under a feeling of hurry and agitation, with that sensibility to sudden impressions, which is

Nervousness.

usually termed nervousness. The mind becomes incapable of any deliberate consideration, and is impressed with horrours, that have no foundation but in a distempered imagination.

Influence of gestation in a carriage.

It is in moderate degrees of this species of affection, that the advantages of carriage exercise are most sensibly felt. The composed serenity of mind, that succeeds to the previous alarm, is described by some persons with a degree of satisfaction, that evinces the decided influence of the remedy. With this steadier tone of mind returns its full power of cool reflection; and if the imagination becomes more alive than usual, its activity is now employed in conceiving scenes, that are amusing and agreeable.

Striking instance of it.

As an instance of direct relief to a circulation labouring from mere fulness of blood, I may adduce that of a person, whose friends, as well as himself, were apprehensive, from the violent and visible throbbing of his heart, of the existence

existence of some organic mischief, and were in some measure alarmed for the consequences.

He was persuaded, and not reluctantly, to go without delay for medical advice; and was accordingly conveyed in a carriage to the house of some physician of eminence, but did not succeed in finding him at home. As the symptoms did not appear to admit of delay, and were at least not aggravated by the motion, it was hoped, that the wished for advice might be obtained at a part of the town, which happened to be at some distance. But the second attempt proved as fruitless as the former, and a third was made with the same event. Since the throbbing had by that time considerably abated, he was contented to postpone any farther efforts to the following day, and directed the carriage homewards. By the time that he returned to his friends, he found, that the motion of travelling over several miles of pavement had apparently removed the complaint. The pulsation of the heart and arteries had subsided to their natural standard, and he congratulated himself, that his search of a remedy had not been ineffectual, although he had been disappointed as to the source, from which he thought he had most reason to expect relief.

Vigour directly given.

If vigour can in any instance be directly given, a man may certainly be said to receive it in the most direct mode, when the important service of impelling forward the circulation of his blood is performed for him by external means. The main spring, or first mover of the system, is thereby, as it were, wound up; and although the several subordinate operations of so complicated a machine cannot be regulated in detail by mere external agency, they must each be performed with greater freedom, in consequene of this general supply of power.

Benefit of gestation in chronical diseases.

In almost every treatise on the subject of chronical diseases are to be found numerous instances of the benefit, produced by the several modes of gestation, which have been most generally adopted; as riding on horseback, in carriages, sea voyages, and swinging. And in many cases, which might be adduced, it has appeared too clear, to admit of a doubt, that the cure of the patient has been owing *solely* to the external agitation of his body, which must

must be allowed, at least, to have had the effect above explained: that of relieving the heart and arteries from a great part of their exertion in propelling the blood, and *may* therefore have contributed to the cure by this means only.

It should be employed in different modes.

The different modes above mentioned are adapted from their nature to different degrees of bodily strength; and if there are cases, in which that which appears most eligible may not suit the situation or circumstances of the patient, it can not be difficult to contrive other means of giving motion, so as least to incommode, and yet to give the greatest relief. A very gentle and long continued, or even incessant motion, may suit some cases better than any more violent and occasional agitation; and in this way, probably, it is, that sea voyages have sometimes been attended with remarkable advantage.

Sea voyages.

VIII.

Method of ascertaining the Value of Growing Timber Trees, at different and distant Periods of Time. By Mr. CHARLES WAISTELL, *of High Holborn.*

(Concluded from p. 193.*)*

Observations respecting Trees of different Lengths in the Bole.

Increase of trees of different lengths of bole at different ages,

TREES that increase annually 12 inches in height and one in circumference, and have boles of different lengths, these boles, if of the undermentioned lengths, increase after the rate of 5 per cent per annum at the ages and heights under-mentioned, and they measure as under, viz,

								Contents.		
			Years old		In.	Ft.		Ft.	in.	p.
Trees with	12	feet boles at	46	their girt	10	at 6	high,	8	4	0
Do.	16	do.	48	do.	10	at 8	do.	11	1	4
Do.	24	do.	52	do.	10	at 12	do.	16	8	0
Do.	32	do.	56	do.	10	at 16	do.	22	2	8
Do.	40	do.	60	do.	10	at 20	do.	27	9	4
Do.	48	do.	64	do.	10	at 24	do.	32	4	0

Whatever

Whatever the lengths of the boles of trees increasing as above may be, the increase is 5 per cent per annum one year after their girt in the middle is 10 inches, but not longer.

But supposing that these trees have grown to 60 years of age, and increased as above-mentioned, their girt and contents at that age would be as under, viz. and at 60 years

							Contents. Ft.	in.	p.
Trees with	16	ft. boles,	13	inches girt at	8	ft high,	18	9	4
Do.	20	do.	12½	do.	10	do.	21	8	5
Do.	24	do.	12	do.	12	do.	24	0	4
Do.	32	do.	11	do.	16	do.	26	10	8
Do.	40	do.	10	do.	20	do.	27	9	4

This table shows, that the advantage to be gained by pruning trees higher than 32 feet is not an object worthy of consideration, if the trees are to be cut down at the age of 60 years. Boles of 32 feet,

And if it should be found, that, the higher a tree is pruned, the slower it swells in the bole, perhaps a 24 feet bole may measure as much at 60 years old as a 32 feet bole. If it increases half an inch in girt in the last 36 years more than the 32 feet bole increases in the same time, it will very nearly equal it in measure. 24 feet,

A 32 feet bole with a top from 20 to 30 feet high, with many large lateral branches, is certainly a much finer object than a forty feet bole with a top only twenty feet high, with a few and small lateral branches: and at sixty years old, the former will have had to increase in the last twenty-eight years only one quarter of an inch in girt, more than the latter, to exceed it in measure, to say nothing of the excess of timber in the larger top and branches. It must, however, be remarked, that at eighty years of age, the forty feet bole will exceed the thirty-two feet bole nearly six feet; and at one hundred years, thirteen feet, provided it swell equally fast in thickness. But unless the trees be oak, fit for the use of the navy, for which an increased price can be had, I imagine few gentlemen would now choose to let their trees stand to eighty years of age, when the increase of their boles will not be four per cent; still fewer would let them stand to one hundred, when the increase will not be three per cent per annum. and 40 feet.

Again

Trees at 60 years of boles from 10 to 50 feet.

Again, let it be supposed, that trees sixty years of age have increased annually, during their growth, fifteen inches in height, and one inch and a half in circumference, the girt and contents of their boles, if of the under-mentioned lengths, will be as under, viz.

							Contents. Ft.	in.	p.
Trees with	20 ft.	boles will be	19½ in.	girt at	10 ft.	high,	52	9	9
Do.	25	do.	18½	do.	12½	do.	61	0	5
Do.	30	do.	18	do.	15	do.	67	6	0
Do.	40	do.	16½	do.	20	do.	75	7	6
Do.	50	do.	15	do.	25	do	78	1	6

Taking it for granted, that the shorter boles will increase faster in thickness than the longer ones, it is reasonable to expect, that the forty feet bole will contain more timber than the fifty feet bole when they are both sixty years old; and if they are both sold at the same rate per foot, the forty feet bole must consequently be more valuable. If, however a higher price can be had for longer boles, this may compensate not only for their deficiency in measure at sixty years of age, but also for their standing beyond the period when they cease paying the common rate of interest for the money they are worth, which I suppose is frequently the case as to tall elm trees, fit for keel pieces, and perhaps beech for ship planking. It is hence evident, that, where the soil is such as will enable trees to grow to a great height, it will be necessary, before we decide how high to prune them, to consider to what purposes the timber can be most advantageously appropriated.

Long boles may be more valuable in some instances.

Elm and beech.

Whatever the lengths of the boles of trees increasing as above may be, their increase is five per cent per annum, one year after their girt in the middle is 15 inches, but not longer.

Trees at 60 years of boles from 24 to 60 feet.

Again, let it be supposed, that trees sixty years of age have increased annually, during their growth, eighteen inches in height, and two inches in circumference, the girt and contents of their boles, if of the undermentioned lengths, will be as under, viz.

							Contents. Ft.	in.	p.
Trees with	24 ft.	boles will be	26 inches	girt at	12 ft.	high,	112	8	0
Do.	30	do.	25	do.	15	do.	130	2	6
Do.	36	do.	24	do.	18	do.	144	0	0
Do.	48	do.	22	do.	24	do.	161	4	0
Do.	60	do.	20	do.	30	do.	166	8	0

Here

Here again we may suppose, that the forty-eight feet bole, by swelling faster than the sixty feet bole, may exceed it in measure at sixty years of age; and this it would do, were the girt increased only half an inch. And if the thirty-six feet bole was increased two inches in girt, it would exceed both the forty-eight and sixty feet boles. But trees of such swift growth are frequently cut down before they are sixty years old. At forty years of age the thirty-six feet bole, if it swell no faster than the forty-eight feet bole, will contain more timber if measured according to the present erroneous method. (The greater disproportion there is between the two ends of a piece of timber, the more disadvantageously it measures, when the girt is taken in the middle.) I suppose that in timber of this swift growth, the longer boles are frequently not worth more per foot than the shorter boles; therefore, in this case, that length of bole should be fixed on, which is likely to measure most at the period when the trees are intended to be felled.

Present method of measuring erroneous.

Whatever the lengths of the boles of trees increasing as above may be, their increase is five per cent per annum, one year after their girt in the middle is 20 inches, but not longer.

It appears from the last observations and calculations, that the annual increase in the boles of trees by their growth ceases to be equal to five per cent per annum some time between forty-six and sixty years of age, according as the boles are shorter or longer.

But it being generally allowed, that oak trees, of a size fit for the navy, require to grow from eighty to one hundred and fifty years, according to the quality of the soil; and it is so stated in the eleventh report of the commissioners appointed to inquire into the state and condition of the woods, forests, and land revenues of the crown; I have therefore been calculating tables, showing what the proportionably advanced prices should be, at different periods, up to one hundred and fifty years, to pay the proprietors for letting their trees stand to those periods. These prices, especially at the later periods, very greatly exceed any that have ever been given. It certainly has been much the interest

Size of oaks for the navy

requires a very high price.

interest of the growers of oak timber to fell it at about sixty years of age, even if they replant the same ground. To let it stand to one hundred and twenty years of age, and sell it at the present prices, their loss would exceed double the whole value of the timber at sixty years of age. Nothing short of a sufficient price will long command a sufficient supply. Owing to too low prices the quantity of large timber on private estates has long been rapidly decreasing; and it will be too late to commence offering reasonable prices for it, when it is all gone, and no oaks left of greater growth than sixty years. To have to wait their growing the second sixty years may bring upon us evils exceeding all calculation.

Loss on their standing 120 years.

Valuations made in October, 1807, of several Plantations in Staffordshire.

Instances of profit on plantations of oak,

The valuations were made of the trees growing within the space of a chain square, being the tenth part of an acre, of the medium growth of each plantation.

In the plantation by the mill wall there are now growing within twenty-two yards square, as under, viz.

	£.	s.	d.		£.	s.	d.
70 oak trees, containing 175 feet, at 2*s.* 3*d.*	19	13	9				
1300 of oak bark, at 12*s.*	7	4	0				
	26	17	9	or, per acre,	268	17	6

The above is part of about four acres planted in 1775, on a strong loamy soil, worth about 20*s.* an acre.

	£.	s.	d.
One pound per ann. forborn 32 years, and improved at 5 per cent compound interest, would amount to	75	6	0

But the value of the timber is more than three times this amount.

The ground was prepared for planting by ploughing.

On

On the east side of Cottage Wood there are now growing, ash, within twenty-two yards square, as under, viz.

	£.	s.	d.		£.	s.	d.
50 ashes, containing 300 feet, at 1s. 6d.	22	10	0				
13 oaks do. 7 do. 2s.	0	14	0				
Bark	0	7	0				
	23	11	0	or, per acre,	235	10	0

The above is part of about two acres planted in 1776, partly on heaps of earth in clay pits, and partly on strong soil upon a deep bed of sand, value about 15s. an acre.

	£.	s.	d.
Fifteen shillings per annum, forborn 31 years, and improved at 5 per cent compound interest would amount to	53	0	0

But the value of the timber is more than four times this amount.

In the clay pits only holes were dug for the plants, but the other part wholly trenched, or double dug with the spade.

In Pickmore Pool Plantation there are now growing, and fir. within twenty-two square yards, as under, viz.

	£.	s.	d.		£.	s.	d.
97 Scotch firs, containing 636 feet*, at 1s. . .	31	16	0	or, per acre,	318	0	0

The last plantation is part of about six acres planted in the springs of 1778 and 9. Much of the soil is a tough peat on gravel or hungry white sand, worth, say, 5s. per acre.

This ground lay between two tenants, who had never cultivated it. They had then nineteen years unexpired of their lease of thirty-one years of this and the adjoining lands, and willingly gave it up to be planted, on condition of having the fences made and kept in good repair.

* This produce is after the rate of 6360 feet an acre, which is about the rate of Table IV.

£. s. d.

Five shillings a year, forborn 29 years, and improved at 5 per cent compound interest, would amount to 15 11 0

But the value of the timber is more than twenty times this amount.

The trees were about two feet high, and planted at two yards distance, in holes dug with the spade, 1210 on an acre. Labour of making the holes and planting the trees cost 1*l.* 6*s.* 10½*d.* per acre.

About 2700 were planted on an acre in the other plantations, where the ground was wholly broken up.

Thinnings pay expenses.

In the remarks on these three plantations, no notice is taken of the thinnings. I am informed by gentlemen who have kept accounts of thinnings, that these have repaid the rent of the land, and every expense, with compound interest, some time before the woods were thirty years old; and the preceding calculations show, that it may be so. And if so, the present value of these plantations is all clear gain.

The valuer of these plantations has bought a good deal of wood out of them; and the prices he has valued at per foot may possibly be a fair value there for such small timber.

Firs on poor ground.

The growth of the firs in the last mentioned plantation is probably as great in that poor ground as it would have been had they been planted on ground of three or four times its value; this must be a powerful inducement to a gentleman to plant all such poor ground in the first instance.

Trees on farms advantageous in screens,

And a few of oaks, ashes, and firs may be raised on almost every farm in screens, that may, by their shelter, increase the value of the farm to the occupier, by increasing the produce, particularly that of grass grounds. In this case the interest of landlord and tenant may be reciprocal; but it is the reverse, where trees are planted in hedge-rows.

not in hedge-rows.

Beneficial on the tops and sides of mountains.

And even the sides and tops of high mountains may be made abundantly more productive of grass, if certain portions of them were surrounded by plantations. These

plantations,

plantations, by breaking the force of cold winds, diminish their chilling effect on the fields the plantations surround, and render the climate on mountains much more mild and genial.

This last kind of improvement will generally be found very greatly to exceed the expectation of the improver, provided it be judiciously planned and executed.

C. WAISTELL.

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*Additional Remarks**.

Great loss is frequently sustained by omitting to thin plantations properly, and in due time, but I am not in possession of facts, to calculate with accuracy what this loss may be; I will however venture to give a short statement of some calculations I have made, as to the loss that would now be sustained, by letting trees grow to a great age. Loss from not thinning plantations properly,

In Miller's Gardener's Dictionary it is stated, that, in a fall of oak timber in Lord Bagot's woods, Mr. Marshall counted the rings of one tree, which was sound at the butt, and found the number to be about 200. Its bole was 22 feet long, and 108 inches in circumference in the middle. Its contents 110 feet, which at 2*s.* amount to 11*l.* I think it was last year, that a fine sound oak tree was cut down, between Shrewsbury and Oswestry in Shropshire, of 300 years of age, and sold by auction for 52*l.* 5*s.*—And under my direction, many oak trees were cut down, some years ago, that could not be less than 300, and some of them probably 400 years of age, and even more. In Hunter's Evelyn's Sylva is given the circumference of 10 trees, not one of which was probably less than 500, and some of them probably 1000 years old. and from letting trees stand too long.

Lord Bagot's tree of 200 years old, above-mentioned, would, at the present price of 3*s.* a foot, be worth 16*l.* 10*s.* Supposing that 3*s.* a foot should continue to be the price of oak timber, for the next 200 years, we will inquire what sum might be raised by growing four oak trees in succession, upon the same spot of ground, each tree to be cut down when 50 years of age, and that their boles

* Trans. of the Soc. of Arts, vol. XXVII, p. 81.

Loss from letting trees stand too long.

should be of the same length as that of Lord Bagot's, viz. 22 feet.

I fix on fifty years of age, as being convenient for my calculation; and nearly the most profitable period at which to cut down trees of 22 feet bole, which have grown at the medium rate of one inch in circumference, and 12 inches in height, annually.

After its 52nd year, *such a bole* ceases increasing after the rate of 5 per cent per annum*; but the whole tree, including the top part above the bole, may continue increasing after that rate until its 61st year†.

I do not fix on 50 years of age as being the most profitable age at which to cut down trees; probably 60 or 70 years of age would in some instances be preferable. Supposing an oak tree has increased, as above-mentioned, its bole of 22 feet would, at 50 years of age, measure 39 inches in circumference at the middle; and one fourth of this, namely 9¾ inches, squared and multiplied into 22 feet, its length, gives 14 feet 6 inches for its contents, which at 3*s.* a foot, its present value, amounts to £2. 3*s.* 6*d.* Supposing £2 3*s.* 6*d.* to be the value of each of the four trees of fifty years of age, grown in succession upon the same spot of ground, in the period of 200 years, we will calculate to what the first three trees would amount, if their value was placed out at compound interest, for the respective terms of 150, of 100, and of 50 years.

£.	s.	d.		£	s.	d.
2	3	6	Accumulating during 150 years, at 5 per cent per annum compound interest, will amount to	3280	0	0
3	3	6	Accumulating as above for 100 years would amount to	286	0	0
2	3	6	Accumulating as above for 50 years would amount to	24	0	0
			Add to the value of the tree to be cut down at the end of 200 years..	2	3	6
			Total amount in 200 years.....	3592	3	6

* See Table 10 of a bole of 24 feet, p. 19.

† See my first Table, p. 28.

And

And carrying forward this calculation, the total amount of the produce in 300 years would amount to.....£472408 0 0

In former times, when the value of oak-woods were estimated by the number of hogs their acorns would fatten, the great age of trees would be of small consideration; but in the present times, I am persuaded, that, if gentlemen, who have many trees standing of the age of 150 years and upwards, would give this subject its due consideration, they will be aware of the immense loss, to which they are voluntarily subjecting themselves—And this great loss is much to be regretted, in a political point of view, especially as the produce of this island is insufficient for its necessary consumption.

Oaks formerly valued for their acorns.

IX.

Remarks on Professor WOOD'S *new Theory of the Diurnal Motion of the Earth round its Axis. In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

I HAVE waited some time in hopes, that Prof. Wood's hypothesis, intended to subvert the Newtonian theory of the tides, would be noticed by some one or other of the able mathematicians in our country. They can scarcely think the production of the American professor so absurd, as to merit no attention; and on the other hand, if its truth be evident, it surely does not deserve to be passed over in silence. Nothing however having yet been said about it in your Journal, I am tempted to ask, whether the prof. have not led himself into an errour by confounding together absolute motion and relative motion, and reasoning on the effects of one from the quantity of the other,

Professor Wood's hypothesis not yet noticed.

Apparent source of his mistake.

That

The centrifugal force not increased in any point of an epicycloid.

That the point of the circle generating an epicycloid, which is farthest from the centre of the circle round which it revolves, moves faster through space, and consequently has a greater absolute velocity, than the opposite point, I am not inclined to dispute. But I presume the motion, that produces centrifugal force, is the rotary motion round that centre, from which the particles of matter have a tendency to fly off: and it appears to me, that, the rotary motion of every point in the circumference of the generating circle round the centre of that circle being the same, its velocity with respect to that centre is uniform, and of course there is no alternate increase and diminution of this velocity, which produces the centrifugal force.

Farther illustration of this.

This is perhaps the simplest mode of considering the subject: but we might take it in another point of view. If we suppose the circle A D B E, in prof. Wood's diagram, vol. XXVI, pl. V, fig. 7, to be moving through space in the direction C *C*, while revolving in the direction A D B; and put *a* for the velocity with which A revolves round C, *b* for the velocity with which B revolves round C, and *c* for the velocity with which the centre, C, is carried in the direction C *C*: then, as the motions of A and C are in the same direction, the absolute velocity of A through space will be $a+c$; and, as the motions of B and C are in opposite directions, the absolute velocity of B will be $b-c$. But the relative velocity of A with respect to C will be simply *a*; because from its absolute velocity, $a+c$, we must deduct the velocity of C, moving in the same direction, and $a+c-c$ obviously $=a$: and again, the relative velocity of B with respect to the centre C must continue $=b$; because, if to its absolute velocity, $b-c$, we add that of C, moving in the opposite direction, we shall have $b-c+c=b$. Now *a* and *b* are clearly equal, because they merely express the velocity, with which two different points in the circumference of the same circle revolve round its centre: and therefore the centrifugal force is not in any way affected by the epicycloidal motion.

I am, Sir,

Your very obedient humble servant,

Nov. 12, 1810. T. NOOT.

Remarks.

REMARKS.

The hypothesis of prof. Wood I believe is not new. I understand the same theory suggested itself to Mr. James Ferguson, who valued himself upon the discovery; but his manuscript was never sent to the press, as, on showing it to some friends, it was thought to be founded on erroneous principles. The preceding letter I insert with pleasure, as it appears to me, to give a simple and satisfactory account of the subject: but, though such is my private opinion, I considered it proper to lay prof. Wood's circular letter before my readers; particularly as I knew that some, who do not rank among the most contemptible mathematicians, had not been able to make up their minds respecting it.

The hypothesis not new,

but not decided on.

X.

An analytical Essay on the Scammonies of Aleppo and Smyrna, with some Observations on the reddening of Litmus by Resins: by Messrs. BOUILLON-LAGRANGE *and* VOGEL*.

THE two sorts of scammony are obtained from the root of a plant, that grows in Syria. The finest and purest scammony is procured by making an incision in the root, and drying in the sun the juice that exudes. But frequently, in order to obtain a larger quantity, the people of Syria and Natolia express the juice, and not only from the root, but from the stalks and leaves also. Often too they adulterate it, by mixing with it the juice of some other milky and acrid plants, as that of the spurges; or increase its weight by a mixture of ashes and other foreign matters. To know that the scammony contains none of these heterogeneous substances, the buyer should break the lumps, choose those that are shining interiorly, and reject those that appear too black, burnt, or containing sand.

Two sorts of scammony how obtained.

Adulterations.

* Annales de Chimie, vol. LXXII, p. 69.

Aleppo

Difference in their appearance.

Aleppo scammony is light, of an ashen gray, shining, transparent in its fracture. That of Smyrna is very compact, heavy, of a darker colour, and more difficult to powder.

Examination of Aleppo scammony.

Aleppo scammony exposed to heat.

When this scammony is pure, it melts entirely on a heated plate of iron, and emits fumes of a nauseating smell.

Treated with water,

Triturated with water it renders it milky. With boiling water it concretes into a lump; the water becomes yellow, and has a bitter taste; but it is neither alkaline, nor acid; which proves, that this substance is not adulterated with ashes, as some authors affirm.

Alcohol at 40° [sp. grav. 0·817] produces a slight precipitate in this aqueous solution; and the acetate of lead occasions a yellow flocculent precipitate soluble in nitric acid.

and with alcohol.

The alcoholic tincture of scammony has a brownish yellow colour. It reddens tincture of litmus; and leaves on evaporation a yellowish white and transparent resin.

Resin.

This resin dissolves entirely in nitric acid, which it colours yellow. The addition of water renders this solution slightly turbid.

Combines with potash,

It is equally soluble in a solution of pure potash, even without heat, when its colour is yellow; but, if heat be employed, it is brown. Water, even in pretty large quantity, does not precipitate any of the resin. If the solution be saturated with muriatic acid, the resin does not separate. This triple compound of resin, acid, and potash, claims the notice of practitioners: perhaps we may thus discover a solvent for resins, that water would not render turbid.

and then with muriatic acid.

Insoluble part.

The part of the scammony that was not soluble in alcohol assumed a gray colour when dry. Treated with boiling water it coloured it yellow, and alcohol occasioned a white flocculent precipitate in it.

Experiments to determine the proportions of its component parts.

To determine the proportion of the constituent principles of Aleppo scammony, we took 100 parts of this substance, and exhausted them by alcohol. The solution was yellow. A gray substance remained, which, when dried, weighed 26.

The

The alcoholic solution was evaporated to a sirupy consistence. Cold water precipitated from it a resin, which formed a homogeneous mass. The supernatant liquor was clear and colourless. Evaporated to dryness a brown matter was obtained, soluble both in water and in alcohol, and precipitable by acetate of lead. This substance appeared to be what is called extract. When dried it weighed 2 parts.

The resinous mass, separated and dried, was yellow, and weighed 60.

The 26 parts insoluble in alcohol were then treated with boiling water. After evaporation a glutinous matter remained, weighing 3 parts, and having all the characters of gum. The remainder consisted of fibres of vegetables and a little silex.

Subjected to dry distillation.

The distillation of Aleppo scammony exhibited nothing remarkable. Its products were a very acid brown liquor, and a light, blackish oil. The coal was black, shining, and compact. It contained the carbonates of potash and lime, alumine, silex, and a little iron.

Examination of Smyrna scammony.

Smyrna scammony less fusible.

The fusion of Smyrna scammony is less complete than that of Aleppo. Instead of concreting into a lump with boiling water, it becomes clotty; but the water poured off has similar qualities.

Treated with water,

and with alcohol.

An equal portion of this scammony, exhausted by boiling alcohol, afforded a tincture of a deeper colour, though containing less resin. By evaporation a brownish, transparent resin was obtained, weighing 28 parts. The matter insoluble in alcohol weighed 66. This residuum, treated with boiling water, coloured it yellow. The solution had a faint sweetish taste; and alcohol produced in it a flocculent precipitate soluble in water. On evaporation it left a thick glutinous matter resembling mucilage, soluble with heat in weak nitric acid, and letting fall on cooling a white pulverulent substance, that had all the characters of mucous acid.

Residuum boiled in water.

Insoluble parts.

In this experiment water took up only 8 parts of the matter insoluble in alcohol. The remainder was subjected to the action of nitric acid assisted by heat, which dissolved

it

it with effervescence. Ammonia added to this solution threw down a precipitate soluble in potash. Potash and oxalate of ammonia too occasioned a precipitate. This residuum therefore, beside vegetable fibres and the substance insoluble in water and alcohol, which appeared to be oxigenized extract, was composed of alumine and carbonate of lime.

This substance, being incinerated, left a whitish powder, soluble in great part with effervescence in muriatic acid. This solution contained alumine, lime, and a little iron. The portion not soluble in muriatic acid, being treated with potash, yielded a siliceous precipitate on the addition of an acid.

Extractive matter.

The water employed to precipitate the resin left after evaporation a brown matter, weighing 5 parts, of a bitter taste, attracting the moisture of the atmosphere, soluble in alcohol, and copiously precipitated from its aqueous solution by acetate of lead. This substance exhibited all the properties of extract.

Component parts of Aleppo scammony,

From this analytical essay therefore it follows, that Aleppo scammony is composed of

Resin	60
Gum	3
Extract	2
Vegetable fibres, earthy matter, &c.	35
	100

and of Smyrna scammony.

and that Smyrna scammony contains

Resin	29
Gum	8
Extract	5
Vegetable fibres, &c.	58
	100

The resins have both apparently the same virtues.

Though the resins obtained from the two sorts of scammony have considerable analogy, yet, as that of the Aleppo is yellow, transparent, and friable, while that of the Smyrna is darker coloured, and more difficult to powder, we thought it would not be useless to ascertain, whether there were any difference in their medicinal properties. In consequence

sequence several physicians undertook to make comparative trials on persons of nearly similar constitutions, but they have not yet observed any difference in their purgative effects.

From the preceding analysis we may conclude, that scammony is a true gum-resin mingled with a little extract. It is true it contains much less gum than the other gum-resins, yet enough to form a milky liquor with water. A gum-resin mixed with extract.

The action of the alcoholic tincture of scammony on litmus led us naturally to examine, whether the property of reddening this blue colour were owing to an acid. None of our experiments having furnished a direct proof of this, we made a comparative trial of some resins, which we subjected to the following experiments. Litmus reddened by resins.

1. *Sandarach.* This resin is converted into a grumous mass by boiling in water. The filtered liquid remains clear; and, when evaporated to a certain point, slightly reddens tincture of litmus; its taste is bitter; it does not alter infusion of violets; it is not precipitated by alcohol, or acetate of lead, which shows, that it contains neither gum nor extract. It is therefore a pure resin. Gum sandarach.

The resin thus treated with boiling water was dissolved in alcohol. This tincture strongly reddened that of litmus, and had no action on sirup of violets.

Powdered sandarach was digested in alcohol, and boiling water poured into the hot filtered solution, which precipitated the resin. The filtered liquid grew turbid on cooling; it had the strong smell of resin of sandarach; its taste was bitter; and its action on the tincture of litmus was so weak, that it could not be supposed to contain a free acid.

2. *Mastic.* This substance exhibits nearly the same phenomena as the preceding; but the resin concretes into a mass in boiling, like turpentine. The water has a bitter taste, and has no action either on litmus or sirup of violets. The resin, on the contrary, strongly reddens tincture of litmus. Mastic.

3. *Olibanum* forms with hot water a thick pap, which is separated from the liquid with difficulty even by filtration. Olibanum.

This

This water has a blackish brown colour, is not precipitated by acetate of lead, and does not alter the colour of litmus; but alcohol throws down a copious precipitate from it, which proves, that this substance is composed of gum and resin.

The alcoholic tincture strongly reddens that of litmus.

If the resins that have most action on the colour of litmus be heated with all due precautions on a sand bath, no acid sublimes.

Treated with lime, according to Scheele's process, no calcareous benzoates are formed.

Various other resinous substances.

4. Lastly, the gum resin ammoniacum, myrrh, elemi, anime, galbanum, tacamahacca, resin of jalap, both prepared by ourselves and that of the shops, Venice turpentine, oil of turpentine, and several other resinous and gum-resinous substances, afforded the same results as those obtained from scammony, sandarach, and olibanum. From these facts it appears still difficult to solve the question, whether the reddening of litmus by resins be owing to the presence of an acid in them.

No proof that this is occasioned by an acid.

If acids alone had the property of reddening vegetable blues, we should not hesitate to admit their existence in resins, though not yet otherwise demonstrated by experiment. As to the infusion of violets not being reddened by resins, this property occurs in the sublimed acid of benzoin, which strongly reddens infusion of litmus, but does not alter the colour of violets. Has this acid, notwithstanding its solubility in water, some analogy to resins? On this we refrain from giving a decided opinion; yet we are inclined to believe, that this substance is a compound of a vegetable acid and a small portion of resin, to which perhaps its solidity is owing. Finally, as all the vegetable acids are soluble in water, it is difficult to ascribe to an acid this property in resins of reddening litmus. It appears more proper therefore, to consider the reddening of litmus as a character of resins, till fresh experiments have proved the contrary.

Benzoic acid perhaps combined with some resin.

Probably it is a character of resins to redden litmus.

SCIENTIFIC

SCIENTIFIC NEWS.

The Elements of Experimental Chemistry, by WILLIAM HENRY, *M. D. F. R. S. &c. The 6th Edition, greatly enlarged; and illustrated with Nine Plates engraved by* LOWRY. *2 vols. 8vo. 1138 p.* Henry's Elements of Chemistry.

FEW of our chemical readers can be unacquainted with the useful work of Dr. H., that first appeared in 1800, under the title of an Epitome of Chemistry, forming a volume of about 200 p. in 12mo; and subsequently increased, through successive editions, to a thick 8vo. The unexampled progress made in the science of chemistry within the last two years has rendered such an addition of new matter necessary, that the bulk of the present edition is more than double that of the last; and as much alteration was requisite on the same account in what had before been written, it may now be considered almost as a new work. Accordingly the author has altered its title, to render it more appropriate to its present state. It would be superfluous to say more of its merits, than that it is worthy of its author; and of a work so long before the public nothing more can be necessary, than to point out what has been done in the present edition. The chapters on Chemical Affinity, and Heat or Caloric, have received copious introductions, explaining their theory and laws. After the chapter on Water follows one entirely new on the Chemical Agencies of Common and Galvanic Electricity. The analysis of the fixed alkalis and ammonia, and an account of their bases, render the chapter on Alkalis almost wholly new; and nearly the same may be said of the following chapter on Earths. The chapter on Acids, which was a mere enumeration of their characteristic qualities in less than a page, now very advantageously occupies seven with a disquisition on their nature and properties. The introduction to chap. viii, on the General Properties of Metals, is completely rewritten, and much enlarged. Bitumens, and the Vegetable Principles of Asparagus, Elm tree

tree gum, and Elecampane, form two new sections in addition to chap. xix. The introduction to chap. xxi, on Animal Substances; the sections on Gelatine, Albumen, and Mucus; and chap. xxiv, of the more complex Animal Products, may be considered as new. Indeed every chapter in Part I has received very considerable additions, particularly that on Vegetable substances; and scarcely a page will be found without some alteration or amendment. In the 2d Part of the work, the Analysis of Minerals and Mineral Waters, and in the 3d, the Application of Tests to various useful Purposes, much less was to be done; yet these have not been left unimproved, particularly in one important point, the detection of arsenic in persons supposed to have been poisoned. In an Appendix are given all the discoveries in chemistry, that occurred during the progress of the work through the press, even up to those of Mr. Davy, that are to appear in the 2d part of the Phil. Trans. for the present year. Five or six new tables have been added to the useful collection in the preceding edition, two or three improved ones have been substituted for some of the former, and a few necessary corrections have been made in others. An additional plate too is given, so that the series now comprehends every article of apparatus essential to the pursuit of experimental chemistry.

Treatise on the doctrine of fluxions applied to military and naval science.

Mr. W. Moore, of the Royal Military Academy, will complete in the year 1811 a Treatise on the Doctrine of Fluxions, with its Application to all the most useful parts of the True Theory of Gunnery; The Motion of Rockets, in different Mediums; the Blowing up of Bridges, Fortifications, &c.; and several other new and important matters connected with Military and Naval Science. The Fluxions will be treated in the most easy manner, that the subject will admit; and the same correct principles observed throughout the performance; a thing which no Author in the English Language, that I am acquainted with, has done. The whole will be printed in 1 Vol. 8vo, and will be particularly adapted for all Military Institutions of eminence.

In

In a late report made to the French Emperor on the arts and manufactures of France it is stated, that a grand improvement has been made in calico printing by the discovery of a permanent green. The English are represented as having offered great rewards and sought in vain for this process, which France has had the honour of discovering, and the advantage of which she alone will enjoy. It is now nearly two years since Mr. Ilett of Stratford took out a patent for a mode of producing permanent greens on linen and cotton by a process in all probability the same as that alluded to in the French report; for the English journals, in which Mr. Ilett's specification was given, must have made his process known in France. Shortly after the introduction of Mr. Ilett's colour, we understand a Mr. Thomson, a calico printer in Lancashire, discovered a mode of producing a permanent green on linen and cotton at one application, by a process entirely different from that of Mr. Ilett, in as much as it is compatible with all the various systems of colours afforded both by madder and weld. This last process supplies a great desideratum in the art of calico printing, and is capable of extensive application.

Permanent green said to be discovered in France,

probably of English origin.

METEOROLOGICAL

METEOROLOGICAL JOURNAL,

For NOVEMBER, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

OCT. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN.	WEATHER. Day.	Night.
28	47°	43°	50°	35°	29·68		Rain	Fair
29	39	36	42	31	·63	·365	Fair	Ditto*
30	38·5	37	42	29·5	·83		Ditto	Ditto
31	35	44·5	44·5	40	30·02		Fair	Cloudy
NOV.								
1	44·5	40·5	47	36	29·74		Rain	Ditto
2	40·5	41	46	38	·79	·045	Ditto	Fair
3	44	44	46	40	·98	·045	Ditto	Cloudy
4	44	41	45	32	·90	·275	Ditto	Fair
5	35·5	39	40·5	36	·35†	·040	Fair*	Ditto*
6	40	40·5	43·5	36	·19	·030	Rain	Fair
7	41	41	42	36·5	28·95	·160	Ditto	Cloudy
8	41·5	42·5	46	35	29·10	·155	Ditto	Ditto
9	39	43	47	40	·48	·215	Fair	Foggy‡
10	46	43·5	48	39·5	28·80§	·540	Rain	Fair
11	45	44·5	46	40·5	29·24	·390	Ditto	Rain
12	44	40·5	45	35	·58	·170	Fair	Fair
13	37·5	39·5	44	35	30·13		Ditto	Ditto
14	39	40	42	40	·11		Rain	Rain
15	51·5	53	54·5	48·5	29·50	·595	Fair	Ditto
16	51	53	54·5	45	·28	·325	Ditto	Rain‖
17	48	47	51	42·5	·35	·070	Ditto	Fair¶
18	45	46	48	45	·54	·025	Fair	Rain
19	47	47	50·5	42	·59	·165	Rain	Cloudy
20	44·5	45·5	50	45	·67	·040	Cloudy	Rain
21	51·5	52	54	45	·43	·250	Rain	Fair
22	48	47	48 5	43	·54	·080	Ditto	Ditto
23	47·5	48·5	50	44	·84	·290	Fair	Rain
24	48	48·5	51	41	·75	·160	Rain	Ditto
25	45	45	50·5	39	·60	·320	Ditto	Fair
26	44	44	46·5	39	·32	·110	Ditto	Rain**

5·280 Inch. noted at 9 A.M. for the preceding 24 hours.

* Snow in the night. † Uncertain. * Intervening fogs. ‡ Stormy night with rain.
§ Barometer at noon 28 61.
‖ Very fine at 11. ¶ Rain at 6 P.M. after fine. ** Evening fair, after 10 rain.

A

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SUPPLEMENT TO VOL. XXVII.

ARTICLE I.

Researches on the Oximuriatic Acid, its Nature and Combinations; and on the Elements of the Muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution. By H. DAVY, *Esq. Sec. R. S. Prof. Chem. R. I. F. R. S. E.* *.

THE illustrious discoverer of the oximuriatic acid considered it as muriatic acid freed from hidrogen †; and the common muriatic acid as a compound of hidrogen and oximuriatic acid; and on this theory he denominated oximuriatic acid dephlogisticated muriatic acid. Discovery of oximuriatic acid.

Mr. Berthollet ‡, a few years after the discovery of Scheele, made a number of important and curious experiments on this body; from which he concluded, that it was composed of muriatic acid gas and oxigen; and this idea for nearly twenty years has been almost universally adopted.

* Philos. Trans. for 1810, p. 231. Communicated to the Royal Society at the request of the managers of the Royal Institution.

† Mem. Acad. Stockholm for 1774, p. 94.

‡ Journal de Physique, 1785, p. 325.

Hidrogen produced from muriatic acid gas.

Dr. Henry, in an elaborate series of experiments, made with the view of decomposing muriatic acid gas, ascertained, that hidrogen was produced from it by electricity; and he attributed the phenomenon to water contained in the gas *.

Muriatic acid not obtainable from the oximuriated without water.

In the Bakerian lecture for 1808 †, I have given an account of the action of potassium upon muriatic acid gas, by which more than one third of its volume of hidrogen is produced; and I have stated, that muriatic acid can in no instance be procured from oximuriatic acid, or from dry muriates, unless water or its elements be present.

Muriatic acid gas contains much water: oximuriatic decomposable only by hidrogen.

In the second volume of the Mémoires d'Arcueil, Messrs. Gay-Lussac and Thenard have detailed an extensive series of facts upon muriatic acid, and oximuriatic acid. Some of their experiments are similar to those I have detailed in the paper just referred to; others are peculiarly their own, and of a very curious kind: their general conclusion is, that muriatic acid gas contains about one quarter of its weight of water; and that oximuriatic acid is not decomposable by any substances but hidrogen, or such as can form triple combinations with it.

Charcoal effects no change in either.

One of the most singular facts that I have observed on this subject, and which I have before referred to, is, that charcoal, even when ignited to whiteness in oximuriatic or muriatic acid gas, by the voltaic battery, effects no change in them; if it has been previously freed from hidrogen and moisture by intense ignition in vacuo.

Existence of oxigen in oximuriatic acid questioned.

This experiment, which I have several times repeated, led me to doubt of the existence of oxigen in that substance, which has been supposed to contain it above all others in a loose and active state; and to make a more rigorous investigation than had hitherto been attempted for its detection.

Oximuriatic acid gas and tin compose the liquor of Libavius,

If oximuriatic acid gas be introduced into a vessel exhausted of air, containing tin; and the tin be gently heated, and the gas in sufficient quantity; the tin and the

* Philos. Trans. for 1800, p. 191: or Journal, 4to series, vol. iv, p. 211.

† Journal, vol. xxiv, p. 95.

gas

gas disappear, and a limpid fluid, precisely the same as Libavius's liquor, is formed. It occurred to me, that if this substance is a combination of muriatic acid and oxide of tin, oxide of tin ought to be separated from it by means of ammonia. I admitted ammoniacal gas over mercury to a small quantity of the liquor of Libavius; it was absorbed with great heat, and no gas was generated; a solid result was obtained, which was of a dull white colour; some of it was heated, to ascertain if it contained oxide of tin; but the whole volatilized, producing dense pungent fumes.

and ammoniacal gas forms with this a solid compound.

Another experiment of the same kind, made with great care, and in which the ammonia was used in great excess, proved that the liquor of Libavius cannot be decompounded by ammonia; but that it forms a new combination with this substance.

I have described, on a former occasion, the nature of the operation of phosphorus on oximuriatic acid; and I have stated, that two compounds, one fluid, and the other solid, are formed in the process of combustion; of which the first, on the generally received theory of the nature of oximuriatic acid, must be considered as a compound of muriatic acid and phosphorous acid, and the other of muriatic acid and phosphoric acid. It occurred to me, that, if the acids of phosphorus really existed in these combinations, it would not be difficult to obtain them, and thus to gain proofs of the existence of oxigen in oximuriatic acid.

Action of oximuriatic acid on phosphorus.

I made a considerable quantity of the solid compound of oximuriatic acid and phosphorus by combustion, and saturated it with ammonia, by heating it in a proper receiver filled with ammoniacal gas, on which it acted with great energy, producing much heat; and they formed a white opaque powder. Supposing that this substance was composed of the dry muriate and phosphate of ammonia; as muriate of ammonia is very volatile, and as ammonia is driven off from phosphoric acid by a heat below redness, I conceived, that, by igniting the product obtained, I should procure phosphoric acid. I therefore introduced some of the powder into a tube of green glass, and heated it to redness, out of the contact of air, by a spirit lamp:

Compound of these saturated with ammonia.

The result not decomposable by a high heat.

but found, to my great surprise, that it was not at all volatile or decomposable at this degree of heat, and that it gave off no gaseous matter.

The circumstance, that a substance composed principally of oximuriatic acid and ammonia should resist decomposition or change at so high a temperature, induced me to pay particular attention to the properties of this new body.

Properties of this compound.

It had no taste nor smell; it did not seem to be soluble, nor did it undergo any perceptible change when digested in boiling water: it did not appear to be acted upon by sulphuric, muriatic, or nitric acids, nor by a strong lixivium of potash. The only processes by which it seemed susceptible of decomposition were combustion, and the action of ignited hidrat of potash. When brought into the flame of a spirit lamp, and made red-hot, it gave feeble indications of inflammation, and tinged the flame of a yellow colour, and left a fixed acid, having the properties of phosphoric acid. When acted on by red-hot hidrat of potash, it emitted a smell of ammonia, burnt where it was in contact with air, and appeared to dissolve in the alkali. The potash which had been so acted upon gave muriatic acid, by the addition of sulphuric acid.

I heated some of the powder to whiteness, in a tube of platina; but it did not appear to alter; and after ignition gave ammonia by the action of fused hidrat of potash.

Ammonia does not decompose phosphuretted or sulphuretted muriatic acid, but forms new compounds with them.

I caused ammonia, made as dry as possible, to act on the phosphuretted liquor of Messrs. Gay-Lussac and Thenard; and on the sulphuretted muriatic liquor of Dr. Thompson; but no decomposition took place; nor was any muriate of ammonia formed when proper precautions were taken to exclude moisture. The results were new combinations; that from the phosphuretted liquor was a white solid, from which a part of the phosphorus was separated by heat, but which seemed no farther decomposable, even by ignition. That from the sulphuretted liquor was likewise solid, and had various shades of colour, from a bright purple to a golden yellow, according as it was more or less saturated with ammonia. But as these compounds did not present the same uniform and interesting

ing properties, as that from the phosphoric sublimate, I did not examine them minutely: I contented myself with ascertaining, that no substance known to contain oxigen could be procured from oximuriatic acid, in this mode of operation.

Oximuriatic acid and ammonia form no water but dry muriat of ammonia and nitrogen.

It has been said, and taken for granted by many chemists, that, when oximuriatic acid and ammonia act upon each other, water is formed. I have several times made the experiment, and I am convinced that this is not the case. When about 15 or 16 parts of oximuriatic acid gas are mixed with from 40 to 45 parts of ammoniacal gas, there is a condensation of nearly the whole of the acid and alkaline gasses, and from 5 to 6 parts of nitrogen are produced; and the result is dry muriate of ammonia.

Action of oximuriatic gas and hidrogen.

Mr. Cruickshank has shown, that oximuriatic acid and hidrogen, when mixed in proportions nearly equal, produce a matter almost entirely condensible by water*; and Messrs. Gay-Lussac and Thenard have stated, that this matter is common muriatic acid gas, and that no water is deposited in the operation. I have made a number of experiments on the action of oximuriatic acid gas, and hidrogen. When these bodies were mixed in equal volumes over water, and introduced into an exhausted vessel and fired by the electric spark, there was always a deposition of a slight vapour, and a condensation of from $\frac{3}{10}$ to $\frac{1}{10}$ of the volume; but the gas remaining was muriatic acid gas. I have attempted to make the experiment in a manner still more refined, by drying the oximuriatic acid and the hidrogen by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatnres; but I have never been able to avoid a slight condensation; though in proportion as the gasses were free from oxigen or water, this condensation diminished.

Oximuriatic acid gas and sulphuretted hidrogen.

I mixed together sulphuretted hidrogen in a high degree of purity and oximuriatic acid gas both dried, in equal volumes: in this instance the condensation was not $\frac{1}{40}$; sulphur, which seemed to contain a little oximuriatic acid,

* Journal, 4to series, vol. v, p. 201 and foll.

was

was formed on the sides of the vessel; no vapour was deposited; and the residual gas contained about $\frac{1}{10}$ of muriatic acid gas, and the remainder was inflammable.

Existence of water in muriatic acid gas questionable.

Messrs. Gay-Lussac and Thenard have proved by a copious collection of instances, that, in the usual cases where oxigen is procured from oximuriatic acid, water is always present, and muriatic acid gas is formed; now, as it is shown, that oximuriatic gas is converted into muriatic acid gas by combining with hidrogen, it is scarcely possible to avoid the conclusion, that the oxigen is derived from the decomposition of water, and, consequently, that the idea of the existence of water in muriatic acid gas is hypothetical, depending upon an assumption which has not yet been proved—the existence of oxigen in oximuriatic acid gas.

Supposed proof of it doubtful.

Messrs. Gay-Lussac and Thenard indeed have stated an experiment, which they consider as proving, that muriatic acid gas contains one quarter of its weight of combined water. They passed this gas over litharge, and obtained so much water; but it is obvious, that in this case they formed the same compound as that produced by the action of oximuriatic acid on lead; and in this process the muriatic acid must loose its hidrogen, and the lead its oxigen; which of course would form water; these able chemists, indeed, from the conclusion of their memoir, seem aware, that such an explanation may be given, for they say that the oximuriatic acid may be considered as a simple body.

Action, on muriatic acid gas, of mercury,

I have repeated those experiments, which led me first to suspect the existence of combined water in muriatic acid, with considerable care; I find, that, when mercury is made to act upon 1 in volume of muriatic acid gas, by voltaic electricity, all the acid disappears, calomel is formed, and about ·5 of hidrogen evolved.

With potassium, in experiments made over very dry mercury, the quantity of hidrogen is always from 9 to 11, the volume of the muriatic acid gas used being 20.

tin, and zinc.

And in some experiments made very carefully by my brother Mr. John Davy, on the decomposition of muriatic acid gas by heated tin and zinc, hidrogen equal to about half its volume was disengaged, and metallic muriates, the same

same as those produced by the combustion of tin and zinc in oximuriatic gas, resulted.

It is evident from this series of observations, that Scheele's view (though obscured by terms derived from a vague and unfounded general theory) of the nature of the oximuriatic and muriatic acids may be considered as an expression of facts; while the view adopted by the French school of chemistry, and which, till it is minutely examined, appears so beautiful and satisfactory, rests, in the present state of our knowledge, upon hypothetical grounds.

Scheele's hypothesis more just than the French.

When oximuriatic acid is acted upon by nearly an equal volume of hidrogen, a combination takes place between them, and muriatic acid gas results. When muriatic acid gas is acted on by mercury, or any other metal, the oximuriatic acid is attracted from the hidrogen, by the stronger affinity of the metal; and an oximuriate, exactly similar to that formed by combustion, is produced.

Oximuriatic acid and hidrogen form muriatic acid; and *vice versa*.

The action of water upon those compounds, which have been usually considered as muriates, or as dry muriates, but which are properly combinations of oximuriatic acid with inflammable bases, may be easily explained, according to these views of the subject. When water is added in certain quantities to Libavius's liquor, a solid crystallized mass is obtained, from which oxide of tin and muriate of ammonia can be procured by ammonia. In this case, oxigen may be conceived to be supplied to the tin, and hidrogen to the oximuriatic acid.

The muriates are compounds of oximuriatic acid with inflammable bases.

The compound formed by burning phosphorus in oximuriatic acid is in a similar relation to water: if that substance be added to it, it is resolved into two powerful acids; oxigen, it may be supposed, is furnished to the phosphorus to form phosphoric acid, hidrogen to the oximuriatic acid to form common muriatic acid gas.

Oximuriatic acid and phosphorus.

None of the combinations of the oximuriatic acid with inflammable bodies can be decomposed by dry acids; and this seems to be the test which distinguishes the oximuriatic combinations from the muriates, though they have hitherto been confounded together. Muriate of potash for instance, if Mr. Berthollet's estimation of its composition approaches towards accuracy, when ignited, is a compound of oximu-

Difference between muriates and oximuriatic combinations.

riatic acid with potassium. Muriate of ammonia is a compound of muriatic acid gas and ammonia; and when acted on by potassium, it is decompounded; the oximuriatic acid may be conceived to combine with the potassium to form muriate of potash, and the ammonia and hidrogen are set free.

Heat and light results of intense agency of combination solely.

The vivid combustion of bodies in oximuriatic acid gas, at first view, appears a reason why oxigen should be admitted in it; but heat and light are merely results of the intense agency of combination. Sulphur and metals, alkaline earths and acids become ignited during their mutual agency; and such an effect might be expected in an operation so rapid as that of oximuriatic acid upon metals and inflammable bodies.

It may be said, that a strong argument in favour of the hypothesis, that oximuriatic acid consists of an acid basis united to oxigen, exists in the general analogy of the compounds of oximuriatic acid and metals to the common neutral salts. But this analogy, when strictly investigated, will be found to be very indistinct; and, even allowing it, it may be applied with as much force to support an opposite doctrine, namely, that the neutral salts are compounds of bases with water; and the metals of bases with hidrogen; and that in the case of the action of oximuriatic acid and metals, the metal furnishes hidrogen to form muriatic acid, and a basis to produce the neutral combination.

New view of the composition of neutral salts.

Quantity of hidrogen evolved from muriatic acid by metals no proof of the presence of water.

That the quantity of hidrogen evolved during the decomposition of muriatic acid gas by metals is the same, that would be produced during the decomposition of water by the same bodies, appears, at first view, an evidence in favour of the existence of water in muriatic acid gas; but as there is only one known combination of hidrogen with oximuriatic acid, one quantity must always be separated. Hidrogen is disengaged from its oximuriatic combination by a metal, in the same manner as one metal is disengaged by another from similar combinations; and of all inflammable bodies that form compounds of this kind, except perhaps phosphorus and sulphur, hidrogen is that which seems to adhere to oximuriatic acid with the least force.

I have

I have caused strong explosions from an electrical jar, to pass through oximuriatic gas, by means of points of platina, for several hours in succession; but it seemed not to undergo the slightest change. Electricity does not decompose oximuriatic gas,

I electrized the oximuriates of phosphorus and sulphur for some hours, by the power of the voltaic apparatus of 1000 double plates: no gas separated, but a minute quantity of hidrogen, which I am inclined to attribute to the presence of moisture in the apparatus employed; for I once obtained hidrogen from Libavius's liquor by a similar operation; but I have ascertained, that this was owing to the decomposition of water, adhering to the mercury; and in some late experiments made with 2000 double plates, in which the discharge was from platina wires, and in which the mercury used for confining the liquor was carefully boiled, there was no production of any permanent elastic matter. or the oximuriates of phosphorus and sulphur.

As there are no experimental evidences of the existence of oxigen in oximuriatic acid gas, a natural question arises, concerning the nature of those compounds, in which the muriatic acid has been supposed to exist combined with much more oxigen than oximuriatic acid, in the state in which it has been named by Mr. Chenevix hyperoxigenized muriatic acid. What is the nature of the hyperoximuriates?

Can the oximuriatic acid combine either with oxigen or hidrogen, and form with each of them an acid compound; of which that with hidrogen has the strongest, and that with oxigen the weakest affinity for bases? for the able chemist, to whom I have just referred, conceives, that hyperoximuriates are decomposed by muriatic acid. Or, is hyperoximuriatic acid the basis of all this class of bodies, the most simple form of this species of matter? Does oximuriatic acid combine with both oxigen and hidrogen? or is hyperoximuriatic acid the base of it and the muriatic?

The phenomena of the composition and decomposition of the hyperoximuriates may be explained on either of these suppositions; but they are mere suppositions unsupported by experiment.

I have endeavoured to obtain the neutralizing acid, which has been imagined to be hyperoxigenised, from hyperoximuriate of potash, by various modes, but uniformly without success. By distilling the salt with dry boracic acid, though Unsuccessful attempts to obtain hyperoximuriatic acid separate.

though a little oximuriatic acid is generated; yet oxigen is the chief gaseous product, and a muriate of potash not decomposable is produced.

The distillation of the orange coloured fluid, produced by dissolving hyperoximuriate of potash in sulphuric acid, affords only oxigen in great excess, and oximuriatic acid.

Facts unfavourable to the supposition of its existence.

When solutions of muriates, or muriatic acid, are electrized in the voltaic circuit, oximuriatic acid is evolved at the positive surface, and hidrogen at the negative surface. When a solution of oximuriatic acid in water is electrized, oximuriatic acid and oxigen appear * at the positive surface, and hidrogen at the negative surface; facts which are certainly unfavourable to the idea of the existence of hyperoxigenised muriatic acid, whether it be imagined a compound of oximuriatic acid with oxigen, or the basis of oximuriatic acid.

Hyperoximuriate of potash probably contains potassium more oxided than in potash.

If the facts respecting the hyperoximuriate of potash, indeed, be closely reasoned upon, it must be regarded as nothing more than as a triple compound of oximuriatic acid, potassium, and oxigen. We have no right to assume the existence of any peculiar acid in it, or of a considerable portion of combined water; and it is perhaps more conformable to the analogy of chemistry, to suppose the large quantity of oxigen combined with the potassium; which we know has an intense affinity for oxigen, and which, from some experiments, I am inclined to believe, is capable of combining directly with more oxigen than exists in potash; than with the oximuriatic acid, which, as far as is known, has no affinity for that substance.

Decomposition of hyperoximuriate of potash by muriatic acid.

It is generally supposed, that a mixture of oximuriatic acid and hyperoximuriatic acid is disengaged, when hyperoximuriate of potash is decomposed by common muriatic acid †; but I am satisfied from several trials, that the gas procured

* The quantity of oximuriatic acid in the aqueous solution is so small, that the principal products must be referred to the decomposition of water. This happens in other instances; the water only is decomposed in dilute solutions of nitric and sulphuric acids.

† If hyperoximuriate of potash be decomposed by nitric or sulphuric acid, it affords oximuriatic acid and oxigen. If it be acted upon

procured in this way, when not mixed with oxigen, unites to the same quantity of hidrogen*, as common oximuriatic acid gas from manganese; and I find, by a careful examination, that the gas disengaged during the solution of platina in a mixture of nitric and muriatic acids, which has been regarded as hyperoximuriatic acid, but which I stated some years ago to possess the properties of oximuriatic acid gas†, is actually this body, owing its peculiar colour to a small quantity of nitromuriatic vapour suspended in it, and from which it is easily freed by washing.

Few substances, perhaps, have less claim to be considered as acid, than oximuriatic acid. As yet we have no right to say that it has been decompounded; and, as its tendency of combination is with pure inflammable matters, it may possibly belong to the same class of bodies as oxigen.

Oximuriatic acid not an acid, but possibly classing with oxigen as an acidifying principle.

May it not in fact be a peculiar acidifying and dissolving principle, forming compounds with combustible bodies, analogous to acids containing oxigen, or oxides, in their properties and powers of combination; but differing from them, in being for the most part decomposable by water? On this idea muriatic acid may be considered as having

upon by muriatic acid, it affords a large quantity of oximuriatic acid gas only. In this last case, the phenomenon seems merely to depend upon the decomposition of the muriatic acid gas by the oxigen loosely combined in the salt.

* This likewise appears from Mr. Cruickshank's experiments. See Nicholson's Journal, Vol. V, 4to, p 206.

† The platina, I find by several experiments, made with great care, has no share in producing the evolution of this gas. It is formed during the production of aqua regia. The hidrogen of the muriatic acid attracts oxigen from the nitric acid. Oximuriatic acid gas is set free, and nitrous gas remains in the solution, and gives it a deep red colour. Nitrous acid and muriatic acid produce no oximuriatic acid gas. Platina, during its solution in perfectly formed aqua regia, gives only nitrous gas and nitrous vapour; and I find, that rather more oximuriatic acid gas is produced, by heating together equal quantities of nitrid acid of 1·45, and muriatic acid of 1·18, when they are not in contact with platina, than when exposed to that metal. The oximuriatic acid gas, produced from muriatic acid by nitric acid, I find combines with about an equal volume of hidrogen by detonation.

The platina has no share in producing this gas.

hidrogen

hidrogen for its basis, and oximuriatic acid for its acidifying principle. And the phosphoric sublimate as having phosphorus for its basis, and oximuriatic acid for its acidifying matter. And Libavius's liquor, and the compounds of arsenic with oximuriatic acid, may be regarded as analogous bodies. The combinations of oximuriatic acid with lead, silver, mercury, potassium, and sodium, in this view would be considered as a class of bodies related more to oxides than acids, in their powers of attraction.

Chemical nomenclature requires a material change.

It is needless to take up the time of this learned society by dwelling upon the imperfection of the modern nomenclature of these substances. It is in many cases connected with false ideas of their nature and composition, and in a more advanced state of the inquiry it will be necessary for the progress of science, that it should undergo material alterations.

Compounds of oximuriatic acid with inflammable substances.

It is extremely probable, that there are many combinations of the oximuriatic acid with inflammable bodies, which have not yet been investigated. With phosphorus it seems capable of combining in at least three proportions; the phosphuretted muriatic acid of Gay-Lussac and Thenard is the compound containing the maximum of phosphorus. The crystalline phosphoric sublimate, and the liquor formed by the combustion of phosphorus in oximuriatic acid gas, disengage no phosphorus by the action of water; the sublimate, as I have already mentioned, affords phosphoric and muriatic acid; and the liquid, I believe, only phosphorous acid and muriatic acid.

The sublimate from the boracic basis gives, I believe, only boracic and muriatic acid, and may be regarded as boracium acidified by oximuriatic acid.

Their decomposition by water a clew to the porportions of elements in oxides, acids, and alkaline earths.

It is evident, that, whenever an oximuriatic combination is decomposed by water, the oxide, or acid, or alkali, or oxidated body formed, must be in the same proportion as the muriatic acid gas, as the oxigen and hidrogen must bear the same relation to each other; and experiments upon these compounds will probably afford simple modes of ascertaining the proportions of the elements in the different oxides, acids, and alkaline earths.

If

If, according to the ingenious idea of Mr. Dalton, hidrogen be considered as 1 in weight, in the proportion it exists in water, then oxigen will be nearly 7·5; and assuming that potash is composed of 1 proportion of oxigen, and 1 of potassium, then potash will be 48, and potassium* about 40·5; and from an experiment which I have detailed in the last Bakerian lecture, on the combustion of potassium in muriatic acid gas, oximuriatic acid will be represented by 32·9, and muriatic acid gas, of course, by 33·9; and this estimation agrees with the specific gravity of oximuriatic acid gas, and muriatic acid gas. From my experiments, 100 cubical inches of oximuriatic acid gas weigh, the reductions being made for the mean temperature and pressure, 74·5 grains; whereas by estimation they should weigh 74·6. Muriatic acid gas I find weighs, under like circumstances, in the quantity of 100 cubic inches, 39 grains; by estimation it should weigh 38·4 grains. Weights of different elements as data.

It is easy from these data, knowing the composition of any dry muriate, to ascertain the quantity of oxide or of acid it would furnish by the action of water, and consequently the quantity of oxigen with which the inflammable matter will combine †.

In

* Supposing potash to contain nearly 15·6 per cent of oxigen.

† I have stated in the last Bakerian lecture, that, during the decomposition of the amalgam from ammonia, 1 in volume of hidrogen to 2 of ammonia is evolved: it is remarkable, that whatever theory of the nature of this extraordinary compound be adopted, there will be a happy coincidence as to definite proportions. If it be supposed that the hidrogen arises from the decomposition of water; then the oxigen, that must be assumed to exist in ammonia, will be exactly sufficient to neutralize the hidrogen in an equal volume of muriatic acid; or if it be said, that ammonium is a compound of 2 of ammonia and 1 of hidrogen in volume, then equal volumes of muriatic acid gas and ammonia will produce the same compound as oximuriatic acid and ammonium, supposing they could be immediately combined. I once thought, that the phenomena of metallization might be explained according to a modified phlogistic theory, by supposing three different classes of metallic bodies: First, the metal of ammonia, in which hidrogen was so loosely combined as to be separable with great ease, Modified phlogistic theory.

produce compounds differing considerably in degree of volatility.

I cannot conclude the subject of the application of these doctrines, without asking permission to direct the attention of the Society to some of the theoretical relations of the facts noticed in the preceding pages.

Extraordinary nature of the compound of oximuriatic acid and ammonia.

That a body principally composed of oximuriatic acid and ammonia, two substances which have been generally conceived incapable of existing together, should be so difficult of decomposition, as to be scarcely affected by any of the agents of chemistry, is a phenomenon of a perfectly new kind. Three bodies, two of which are permanent gases, and the other of which is considerably volatile, form, in this instance, a substance neither fusible nor volatile at a white heat. It could not have been expected, that ammonia would remain fixed at such a temperature; but that it should remain fixed in combination with oximuriatic acid would have appeared incredible, according to all the existing analogies of chemistry. The experiments, on which these conclusions are founded, are, however, uniform in their results; and it is easy to repeat them. They seem to show, that the common chemical proposition, that complexity of composition is uniformly connected with facility of decomposition, is not well founded. The compound of oximuriatic acid, phosphorus, and ammonia, resembles an oxide, such as silex, or that of columbium in its general chemical characters, and is as refractory when treated by common reagents; and except by the effects of combustion, or the agency of fused potash, its nature could not be detected by any of the usual methods of analysis. Is it not likely, reasoning from these circumstances, that many of the substances, now supposed to be elementary, may be reduced into simpler forms of matter? And that an intense attraction, and an equilibrium of attraction, may give to a compound, containing several constituents, that refractory character, which is generally attributed to unity of constitution, or to the homogeneous nature of its parts?

Complexity of composition not always connected with facility of decomposition.

Other compounds of the oximuriatic acid,

Beside the compound of the phosphoric sublimate and ammonia, and the other analogous compounds which have been

been referred to, it is probable, that other compounds of like nature may be formed of the oxides, alkalis, and earths, with the oximuriatic combinations, or of the oximuriatic compounds with each other; and should this be the case, the more refined analogies of chemical philosophy will be extended by these new, and, as it would seem at first view, contradictory facts. For if, as I have said, oximuriatic acid gas be referred to the same class of bodies as oxigen gas, then, as oxigen is not an acid, but forms acids by combining with certain inflammable bodies, so oximuriatic acid, by uniting to similar substances, may be conceived to form either acids, which is the case when it combines with hidrogen, or compounds like acids or oxides, capable of forming neutral combinations, as in the instances of the oximuriates of phosphorus and tin.

which appears to be analogous with oxigen.

Like oxigen, oximuriatic acid is attracted by the positive surface in voltaic combinations; and on the hypothesis of the connection of chemical attraction with electrical powers, all its energies of combination correspond with those of a body supposed to be negative in a high degree.

And in most of its compounds, except those containing the alkaline metals, which may be conceived in the highest degree positive, and the metals with which it forms insoluble compounds, it seems still to retain its negative character.

(To be concluded in our next.)

II.

Observations upon Luminous Animals. By J. Macartney, *Esq. Communicated by* Everard Home, *Esq. F. R. S.**

THE property, which certain animals possess of emitting light, is so curious and interesting, that it has attracted the attention of naturalists in all ages. It was particularly noticed by Aristotle and Pliny among the ancients; and the publications of the different learned societies in Europe

Luminous animals have attracted much attention,

* Philos. Trans. for 1810, p. 253.

 contain

contain numerous memoirs upon the subject. Notwithstanding the degree of regard bestowed upon the history of luminous animals, it is still very imperfect; the power of producing light appears to have been attributed to several creatures which do not possess it; some species, which enjoy it in an eminent degree, have been imperfectly described, or entirely unobserved: the organs which afford the light in certain animals have not been examined by dissection; and lastly, the explanations that have been given of the phenomena of animal light are unsatisfactory, and in some instances palpably erroneous.

but our knowledge of them very imperfect.

The author has long studied them,

As this subject forms an interesting part of the history of organized beings, I have for some years availed myself of such opportunities as occurred for its investigation. Having communicated the result of some of my researches to the Right Honourable Sir Joseph Banks, he immediately offered me his assistance with that liberality, which so eminently distinguishes him as a real lover of science. I am indebted to him for an inspection of the valuable journal he kept during his voyage with Captain Cook; for permission to copy the original drawings, in his possession, of those luminous animals discovered in both the voyages of Cook; and for some notes upon the luminous appearance of the sea, that were presented to him by Captain Horsburg, whose accuracy of observation is already known to this learned Society.

and received valuable communications from Sir. J. Banks.

Plan of the paper.

In the following paper, I shall first examine the grounds on which the property of showing light has been ascribed to certain animals, that either do not possess it, or in which its existence is questionable. I shall next give an account of some luminous species, of which some have been inaccurately described, and others quite unknown. I shall endeavour to explain from my own observations, and the information communicated to me by others, many of the circumstances attending the luminous appearance of the sea. I shall then describe the organs employed for the production of light in certain species; and lastly, I shall review the opinions which have been entertained respecting the nature and original of animal light, and relate the experiments I have made for the purpose of elucidating this part of the subject.

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The property of emitting light has been reported to belong to several fishes, more particularly the mackarel, the moonfish (tetraodon mola), the dorado, mullet, sprat, &c. Luminousness ascribed erroneously to certain fishes;

Mr. Bajon observed during the migration of the doradoes, &c., that their bodies were covered with luminous points. These however proved upon examination to be minute spherical particles, that adhered to the surface of these fishes; and, he adds, appeared to be precisely the same sort of points, that illuminated the whole of the sea at the time. They were therefore in all probability the minute kind of medusa, which I shall have occasion to describe hereafter.

Godeheu de Riville states, in a paper sent to the Academy of Sciences at Paris, that, on opening the scomber pelamis while alive, he found in different parts of its body an oil which gave out much light: but it should be observed, that Riville had a particular theory to support, for which this fact was very convenient; and that other parts of his memoir bear marks of inaccuracy. It may be added, that, if the oil of fishes were usually luminous, which Riville supposed, it would be almost universally known, instead of resting on a solitary observation.

As far as I am able to determine from what I have seen, the faculty of exhibiting light during life does not belong to the class of fishes. It appears probable, that some fishes may have acquired the character of being luminous, from evolving light soon after death. (but no fishes exhibit light while living;)

Some species of lepas, murex, and chama, and some starfish have been said to possess the power of shining; and the assertion has been repeated by one writer after another, but without quoting any authority. to some vermes;

Brugueire upon one occasion saw, as he supposed, common earthworms in a luminous state; all the hedges were filled with them; he remarked, that the light resided principally in the posterior part of the body*. earthworms;

Flaugergues pretended to have seen earthworms luminous in three instances; it was at each time in October; the

* Journal d'Histoire Naturelle, Tom. II.

body shone at every part, but most brilliantly at the genital organs*.

Notwithstanding this concurrence of testimony, it is next to impossible, that animals, so frequently before our eyes as the common earthworm, should be endowed with so remarkable a property, without every person having observed it. If they only enjoyed it during the season for copulation, still it could not have escaped notice, as these creatures are usually found joined together in the most frequented paths, and in garden walks.

the water flea; In different systems of natural history, the property of shining is attributed to the cancer pulex. The authorities for this opinion are Hablitzl, and Thules and Bernard. The former observed upon one occasion a cable that was drawn up from the sea exhibit light, which upon closer inspection was perceived to be covered by these insects †. Thules and Bernard reported, that they met with a number of this species of cancer on the borders of a river entirely luminous ‡. I am nevertheless disposed to question the luminous property of the cancer pulex, as I have often had the animal in my possession, and never perceived it emit any light.

and the scolopendra phosphorea. The account given by Linneus of the scolopendra phosphorea is so improbable and inconsistent, that one might be led to doubt this insect's existence, particularly as it does not appear to have been ever seen, except by Ekeberg, the captain of an East Indiaman, from whom Linneus learnt its history.

I now proceed to the description of those luminous animals, that have been discovered by the Right Honourable Sir Joseph Banks, Captain Horsburg, and myself.

Two luminous marine animals discovered by Sir J. Banks. On the passage from Madeira to Rio de Janeiro, the sea was observed by Sir Joseph Banks to be unusually luminous, flashing in many parts like lightning. He directed some of the water to be hauled up, in which he discovered two kinds of animals, that occasioned the phenomenon; the one, a

* Journal de Physique, Tome XVI.

† Hablitzl ap. Pall. n. Nord. Beytr. 4, p. 396.

‡ Journal de Physique, Tome XXVIII.

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crustaceous insect, which he called the cancer fulgens; the other, a large species of medusa, to which he gave the name of pellucens.

Cancer fulgens.

The cancer fulgens bears some resemblance to the common shrimp; it is however considerably less. The legs are furnished with numerous setæ. The light of this animal, which is very brilliant, appears to issue from every part of the body. See it Pl. IX, fig. 1, of the natural size, and magnified at fig. 2.

Medusa pellucens.

The medusa pellucens measures about six inches across the crown or umbella; this part is marked by a number of opake lines, that pass off from the centre to the circumference. The edge of the umbella is divided into lobules, which succeed each other, one large and two small ones alternately. From within the margin of the umbella there are suspended a number of long cord-shaped tentacula. The central part of the animal is opake, and furnished with four thick irregularly shaped processes, which hang down in the midst of the tentacula. See fig. 3.

This zoophyte is the most splendid of the luminous inhabitants of the ocean. The flashes of light emitted during its contractions are so vivid, as to affect the sight of the spectator.

Shining of the sea observed by captain Horsburg.

In the notes communicated to Sir Joseph Banks by Captain Horsburg, he remarks, that the luminous state of the sea between the tropics is generally accompanied with the appearance of a great number of marine animals of various kinds upon the surface of the water: to many of which he does not, however, attribute the property of shining. At other times, when the water which gave out light was examined, it appeared only to contain small particles of a dusky straw colour, which dissolved with the slightest touch of the finger. He likewise observes, that in Bombay during the hot weather of May and June, he has frequently seen the edges of the sea much illuminated by minute sparkling points.

Luminous insect found by him.

At sunrise, on April 12, 1798, in the Arabian sea, he perceived several luminous spots in the water, which conceiving to be animals, he went in the boat and caught one. It proved to be an insect somewhat resembling in appearance

ance the woodlouse, and was about one third of an inch in length. When viewed with the microscope, it seemed to be formed by sections of a thin crustaceous substance. During the time that any fluid remained in the animal, it shone brilliantly like the fire fly.

Another.

In the month of June in the same year, he picked up another luminous insect on a sandy beach, which was also covered with a thin shell, but it was of a different shape, and a larger size than the animal taken in the Arabian sea.

Both monoculi.

By comparing the above description with an elegant pen and ink drawing, which was made by Captain Horsburg, and accompanied his paper, I have no doubt, that both these insects were monoculi; the first evidently belongs to the genus limulus of Muller; I shall therefore beg leave to distinguish it by the name of limulus noctilucus.

Luminous vermes discovered by the author.

My pursuits, and the state of my health, having frequently led me to the coast, I have had many opportunities of making observations upon the animals, which illuminate our own seas. Of these I have discovered three species: one of which is a beroe not hitherto described by authors; another agrees so nearly with the medusa hemispherica, that I conceive it to be the same, or at least a variety of that species; the third is a minute species of medusa, which I believe to be the luminous animal, so frequently seen by navigators, although it has never been distinctly examined or described.

Minute luminous medusæ described.

I first met with these animals in the month of October 1804, at Herne Bay, a small watering place upon the northern coast of Kent. Having observed the sea to be extremely luminous for several nights, I had a considerable quantity of the water taken up. When perfectly at rest, no light was emitted, but on the slightest agitation of the vessel in which the water was contained, a brilliant scintillation was perceived, particularly towards the surface; and when the vessel was suddenly struck, a flash of light issued from the top of the water, in consequence of so many points shining at the same moment. When any of these sparkling points were removed from the water, they no longer yielded any light. They were so transparent, that in the air they appeared like globules of water. They were

were more minute than the head of the smallest pin. Upon the slightest touch, they broke and vanished from the sight. Having strained a quantity of the luminous water, a great number of these transparent corpuscles were obtained upon the cloth; and the water, which had been strained, did not afterward exhibit the least light. I then put some sea water, that had been rendered particularly clear by repeated filtrations, into a large glass; and having floated in it a fine cloth, on which I had previously collected a number of luminous points, several of them were liberated, and became distinctly visible in their natural element, by placing the glass before a piece of dark coloured paper. They were observed to have a tendency to come to the surface of the water, and after the glass was set by for some time, they were found congregated together, and when thus collected in a body, they had a dusky straw colour, although individually they were so transparent, as to be perfectly invisible, except under particular circumstances. Their substance was indeed so extremely tender and delicate, that they did not become opaque in distilled vinegar or alcohol, until immersed in these liquors for a considerable time.

On examining these minute globules with the microscope, I found that they were not quite perfect spheres, but had an irregular depression on one side, which was formed of an opaque substance, that projected a little way inwards, producing such an appearance as would arise from tying the neck of a round bag, and turning it into the body.

The motions of these creatures in the water were slow and graceful, and not accompanied by any visible contraction of their bodies. After death they always subsided to the bottom of the vessel.

From the sparkling light afforded by this species, I shall distinguish it by the name of medusa scintillans. Medusa scintillans.

The night following that, on which I discovered the preceding animal, I caught the two other luminous species. One of these I shall call the beroe fulgens. Beroe fulgens described.

This most elegant creature is of a colour changing between purple, violet, and pale blue; the body is truncated before, and pointed behind; but the form is difficult to assign,

assign, as it is varied by partial contractions, at the animal's pleasure. I have represented the two extremes of form, that I have seen this creature assume: the first is somewhat that of a cucumber, which, as being the one it takes when at rest, should perhaps be considered as its proper shape: the other resembles a pear, and is the figure it has in the most contracted state. The body is hollow, or forms internally an infundibular cavity, which has a wide opening before, and appears also to have a small aperture posteriorly, through which it discharges its excrement. The posterior two thirds of the body are ornamented with eight longitudinal ciliated ribs, the processes of which are kept in such a rapid rotatory motion, while the animal is swimming, that they appear like the continual passage of a fluid along the ribs. The ciliated ribs have been described by Professor Mitchell as arteries, in a luminous beroe, which I suspect was no other than the species I am now giving an account of.

Light emitted by it.

When the beroe fulgens swam gently near the surface of the water, its whole body became occasionally illuminated in a slight degree; during its contractions, a stronger light issued from the ribs, and when a sudden shock was communicated to the water, in which several of these animals were placed, a vivid flash was thrown out. If the body were broken, the fragments continued luminous for some seconds, and being rubbed on the hand, left a light like that of phosphorus; this however, as well as every other mode of emitting light, ceased after the death of the animal.

Hemispherical medusa.

The hemispherical species of medusa, that I discovered, had a very faint purple colour. The largest that I found, measured about three quarters of an inch in diameter. The margin of the umbella was undivided, and surrounded internally by a row of pale brown spots, and numerous small twisted tentaculæ: four opaque lines crossed in an arched manner from the circumference towards the centre of the animal: an opaque irregular shaped process hung down from the middle of the umbella; when this part was examined with a lens of high powers, I discovered that it was inclosed in a sheath in which it moved, and that the extremity

extremity of the process was divided into four tentacula, covered with little cups or suckers, like those on the tentacula of the cuttlefish.

This species of medusa bears a striking resemblance to the figures of the medusa hemispherica, published by Gronovius and Muller; indeed it differs as little from these figures, as they do from each other. Its luminous property, however, was not observed by these naturalists; which is the more extraordinary, as Muller examined it at night, and says it is so transparent, that it can only be seen with the light of a lamp. If it should be still considered as a distinct species, or as a variety of the hemispherica, I would propose to call it the medusa lucida.

Resembles those of Gronovius and Muller.

In this species, the central part and the spot round the margin are commonly seen to shine on lifting the animal out of the water into the air, presenting the appearance of an illuminated wheel; and when it is exposed to the usual percussion of the water, the transparent parts of its body are alone luminous.

Mode of its shining.

In the month of September 1805, I again visited Herne Bay, and frequently had opportunities of witnessing the luminous appearance of the sea. I caught many of the hemispherical and minute species of medusa, but not one of the beroe fulgens. I observed, that these luminous animals always retreated from the surface of the water, as soon as the moon rose. I found also, that exposure to the day light took away their property of shining, which was revived by placing them for some time in a dark situation.

These animals retreated from the surface of the sea when the moon rose, and did not shine in day-light.

In that season I had two opportunities of seeing an extended illumination of the sea, produced by the above animals. The first night I saw this singular phenomenon was extremely dark, many of the medusa scintillans, and medusa homispherica had been observed at low water, but on the return of the tide, they had suddenly disappeared. On looking towards the sea, I was astonished to perceive a flash of light of about six yards broad, extend from the shore, for apparently the distance of a mile and a half along the surface of the water. The second time that I saw this sort of light proceed from the sea, it did not take the same form, but was diffused over the surface of the

Large flashes of light on the sea from them.

waves

waves next the shore, and was so strong, that I could for the moment distinctly see my servant, who stood at a little distance from me; he also perceived it, and called out to me at the same instant. On both these occasions the flash was visible for about four or five seconds, and although I watched for it a considerable time, I did not see it repeated.

Diffused luminous appearance of the sea.

A diffused luminous appearance of the sea, in some respects different from what I have seen, has been described by several navigators.

Godeheu de Riville saw the sea assume the appearance of a plain of snow on the coast of Malabar*.

Other similar appearances at particular times.

Captain Horsburg, in the notes he gave to Sir Joseph Banks, says, there is a peculiar phenomenon sometimes seen within a few degrees distance of the coast of Malabar, during the rainy monsoon, which he had an opportunity of observing. At midnight the weather was cloudy, and the sea was particularly dark, when suddenly it changed to a white flaming colour all around. This bore no resemblance to the sparkling or glowing appearance he had observed on other occasions in seas near the equator, but was a regular white colour, like milk, and did not continue more than ten minutes. A similar phenomenon, he says, is frequently seen in the Banda sea, and is very alarming to those, who have never perceived or heard of such an appearance before.

Striking instance of this phenomenon,

This singular phenomenon appears to be explained by some observations communicated to me by Mr. Langstaff, a surgeon in the city, who formerly made several voyages. In going from New Holland to China, about half an hour after sunset, every person on board was astonished by a milky appearance of the sea: the ship seemed to be surrounded by ice covered with snow. Some of the company supposed they were in soundings, and that a coral bottom gave this curious reflection, but on sounding with 70 fathoms of line no bottom was met with. A bucket of water being hauled up, Mr. Langstaff examined it in the dark, and discovered a great number of globular bodies, each about the size of a pin's head, linked together. The

* Mém. Etrang. de l'Acad. des Sc. Tom. 3.

chains

chains thus formed did not exceed three inches in length, and emitted a pale phosphoric light. By introducing his hand into the water, Mr. Langstaff raised upon it several chains of the luminous globules; which were separated by opening the fingers, but readily reunited on being brought again into contact, like globules of quicksilver. The globules, he says, were so transparent, that they could not be perceived when the hand was taken into the light.

This extraordinary appearance of the sea was visible for two nights. As soon as the moon exerted her influence, the sea changed to its natural dark colour, and exhibited distinct glittering points, as at other times. The phenomenon, he says, had never been witnessed before by any of the company on board, although some of the crew had been two or three times round the globe.

which continued for two nights, but was dissipated by the moonlight.

I consider this account of Mr. Langstaff very interesting and important, as it proves, that the diffused light of the sea is produced by an assemblage of minute medusæ on the surface of the water.

In June 1806, I found the sea at Margate more richly stored with the small luminous medusæ, than I have ever seen it. A bucket of the water being set by for some time, the animals sought the surface, and kept up a continual sparkling, which must have been occasioned by the motions of individuals, as the water was perfectly at rest. A small quantity of the luminous water was put into a glass jar, and on standing some time, the medusæ collected at the top of the jar, and formed a gelatinous mass, one inch and a half thick, and of a reddish or mud colour, leaving the water underneath perfectly clear.

Number of the small luminous medusæ caught at Margate.

In order to ascertain if these animals would materially alter their size, or assume the figure of any other known species of medusa, I kept them alive for 25 days, by carefully changing the water in which they were placed; during which time, although they appeared as vigorous as when first taken, their form was not in the slightest degree altered, and their size but little increased. By this experiment I was confirmed in the opinion of their being a distinct species, as the young actiniæ and medusæ exhibit the form of the parent in a much shorter period than the above.

They did not alter their form, and scarcely their size in 25 days.

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Beroe fulgens of various sizes.

In September, 1806, I took at Sandgate a number of the beroe fulgens, but no other species: they were of various dimensions, from the full size down to that of the medusa scintillans: they could however be clearly distinguished from the latter species, by their figure.

Medusa scintillans on various parts of our coasts.

Since that time, I have frequently met with the medusa scintillans on different parts of the coast of Sussex, at Tenby, and at Milford haven. I have likewise seen this species in the bays of Dublin and Carlingford in Ireland.

Number of the beroe fulgens caught.

In the month of April, last year, I caught a number of the beroe fulgens in the sea at Hastings: they were of various sizes, from about the half of an inch in length, to the bulk of the head of a large pin. I found many of them adhering together in the sea; some of the larger sort were covered with small ones, which fell off when the animals were handled; and, by a person unaccustomed to observe these creatures, would have been taken for a phosphoric substance. On putting a number of them into a glass, containing clear sea water, they still showed a disposition to congregate upon the surface. I observed, that, when they adhered together, they showed no contractile motion in any part of their body, which explains the cause of the pale or white colour of the diffused light of the ocean. The flashes of light, which I saw come from the sea at Herne bay, were probably produced by a sudden and general effort of the medusæ to separate from each other, and descend in the water.

Cause of the diffused light on the ocean,

and of the flashes in Herne bay.

Medusa scintillans abounds in Milford havent

The medusa scintillans almost constantly exists in the different branches of Milford haven, that are called pills. I have sometimes found these animals collected in such vast numbers in those situations, that they bore a considerable proportion to the volume of the water in which they were contained: thus, from a gallon of sea water in a luminous state I have strained above a pint of these medusæ. I have found the sea under such circumstances to yield me more support in swimming, and the water to taste more disagreeably than usual; probably the difference of density, that has been remarked at different times in the water of the sea, may be referred to this cause.

Probably affect the density of the sea.

The most frequent source of the luminous-

All my own observations lead me to conclude, that the medusa scintillans is the most frequent source of the light

of the sea around this country; and by comparing the accounts of others with each other, and with what I have myself seen, I am persuaded, that it is so likewise in other parts of the world. Many observers appear to have mistaken this species for the nereis noctiluca, which was very natural, as they were prepossessed with the idea of the frequent existence of the one, and had no knowledge of the other. Some navigators have actually described this species of medusa, without being aware of its nature. Mr. Bajon, during his voyage from France to Cayenne, collected many luminous points in the sea, which he says, when examined by a lens, were found to be minute spheres. They disappeared in the air. Doctor Le Roy, in sailing from Naples to France, observed the sparkling appearance of the sea, which is usually produced by the medusa scintillans. By filtering the water, he separated luminous particles from it, which he preserved in spirit of wine: they were, he says, like the head of a pin, and did not at all resemble the nereis noctiluca, described by Vianelli; their colour approached a yellow brown, and their substance was extremely tender, and fragile. Notwithstanding this striking resemblance to the medusa scintillans, Le Roy, in consequence of a preconceived theory, did not suppose what he saw were animals, but particles of an oily or bituminous nature *.

ness of the sea. Mistaken for the nereis noctiluca by some; and described by others, who were not aware of its nature.

The minute globules, seen by Mr. Langstaff in the Indian ocean, were, I think, in all probability, the scintillating species of medusa: and on my showing him some of these animals I have preserved in spirits, he entertained the same opinion.

Professor Mitchell, of New York, found the luminous appearance on the coast of America, to be occasioned by minute animals, that, from his description, plainly belonged to this species of medusa, notwithstanding which, he supposed them to be a number of the nereis noctiluca †.

* Observ. sur une Lumière produite par L'Eau de la Mer. Mém. Etrang. des Sc.

† Phil. Mag. Vol. X, p. 20.

The

The luminous animalcule of Forster.

The luminous animalcule, discovered by Forster off the Cape of Good Hope, in his voyage round the world, bears so strong a resemblance to the medusa scintillans, that I am much disposed to believe them the same. He describes his animalcule as being a little gelatinous globule, less than the head of a pin; transparent, but a little brownish in its colour; and of so soft a texture, that it was destroyed by the slightest touch. On being highly magnified, he perceived on one side a depression, in which there was a tube that passed into the body, and communicated with four or five intestinal sacs. The pencil drawings he made on the spot are in the possession of Sir Joseph Banks, by whose permission engravings from them are subjoined to this paper. By comparing these with the representations of the medusa scintillans, and some of this species rendered visible, by being a long time preserved in spirits, which I have laid before this learned society, it will be found, that the only difference between Forster's animalcule, and the medusa scintillans, is in the appearance of the opaque parts, shown in the microscopic views.

Luminousness of the sea erroneously ascribed to various causes.

Many writers have ascribed the light of the sea to other causes than luminous animals. Martin supposed it to be occasioned by putrefaction: Silberschlag believed it to be phosphoric: professor J. Mayer conjectured, that the surface of the sea imbibed light, which it afterward discharged. Bajon and Gentil thought the light of the sea was electric, because it was excited by friction. Forster conceived, that it was sometimes electric, sometimes caused from putrefaction, and at others by the presence of living animals. Fougeroux de Bondaroy believed, that it came sometimes from electric fires, but more frequently from the putrefaction of marine animals and plants.

I shall not trespass on the time of the Society, to refute the above speculations; their authors have left them unsupported by either arguments or experiments, and they are inconsistent with all ascertained facts upon this subject.

(To be concluded in our next.)

III. *Note*

III.

Note on the Water contained in fused Soda. By Mr. J. E. Berard *.

Water in soda after fusion.

Mr. d'Arcet had found, that pure alkalis, after being fused, contained water, and he estimated the quantity in soda as high as 28 per cent †. An analysis, which I made some time after in a different mode, gave me but 18·86.

Mr. d'Arcet gives the proportions of subcarbonate of soda different from the author,

The result of Mr. d'Arcet is founded on the analysis of the subcarbonate of soda. For this salt too he has given proportions different from those I found ‡. Not knowing the particulars of Mr. d'Arcet's experiments, I ascribed the difference of our results respecting the water in soda to the difference in these proportions. In confirmation of his opinion however, he has just published these experiments; and has made several objections to my method of analysing the subcarbonate, which I shall endeavour to remove.

and makes to his method

two objections.

He considers the solution in acids as an inaccurate mode; 1st, because the solution retains carbonic acid: 2dly, because the gas evolved carries off water with it.

These errours tend to balance each other. The first must be of very small amount;

It may be observed, that, these sources of errour being opposite in their effects, they in great measure counteract each other: but we shall see what is their extent. The temperature amounts at most to 20° [68°F], when the carbonate decomposed by the acid is already in solution. Now at this temperature water under the pressure of the atmosphere alone would not dissolve any thing like its own bulk of carbonic acid gas. But this quantity may be wholly neglected, when we consider, that a large excess of sulphuric acid is added, and the solution strongly shaken.

* Annales de Chim. vol. lxxii, p. 96.

† Ann. de Chim. vol. lxviii, p. 175; or Journal, p. 31 of the present vol.

‡ For the component parts of different salts by Mr. Berard see Journal, vol. xxvi, p. 206.

As

and the same is true of the second.

As to the water carried off by the gas, when the experiment is performed with due caution in proper vessels, the gas only carries with it hygrometrical water. Now this quantity may easily be calculated. The gas in my experiment weighed 4·195 gram. [64·77 grs.]: consequently its volume, at 20° [68°F.] and 0·75 met. [29·5 in.] pressure, was 2·3 lit. [4·8 pints]; and the weight of the aqueous vapour = 4·2 cent. [0·65 of a gr.]. If this quantity be diminished by the very small quantity of carbonic acid retained in the solution*, we shall have the extent of the errour, to which this analysis is liable; for the process is so simple, that on repeating it we obtain almost precisely the same quantities. I conceive it much to be wished, that all analyses were susceptible of this precision: besides, I have confirmed it by other experiments. I reduced the subcarbonate to muriate and sulphate, and the results I obtained were found to agree with the analysis I had adopted.

Objections to the mode of determining the water in soda.

I now proceed to the objections which Mr. d'Arcet has made to my method of determining the water in soda, and which consist in this, that the component parts of muriate of silver are not accurately determined; and that this salt is soluble in the waters of elutriation.

Proportions of muriate of silver ascertained.

Chenevix, Zaboade, Proust, Bucholz, Rose, and others, have successively determined the proportions of muriate of silver. The quantity of acid contained in this salt varies in their results from 17 to 18. Rose and Bucholz make it 17·5, which is the proportion I have adopted. The late experiments of Gay-Lussac carry it to 18. When the proportions assigned to a salt vary only half a hundredth part, they may be fairly considered as ascertained.

This muriate is among the most insoluble salts.

With regard to the solubility of muriate of silver, I believe we may affirm, that it is one of the most insoluble salts employed in analysis, when the liquid is neutral and contains very little of any other salt. It even requires a pretty considerable excess of acid, to dissolve any notable

* There is still another circumstance, that tends to diminish the weight of the carbonic acid: this is, the gas evolved expels great part of the atmospheric air contained in the empty part, and takes its place.

quantity

quantity. Nitrate of silver is such a powerful test of muriatic acid, that it detects extremely small quantities. Kirwan found, that one part of muriatic acid diluted with 108333 of water could be detected by nitrate of silver*. Does muriate of lime indicate such imperceptible quantities of carbonic acid, particularly in a liquid that is not neutral?

These observations seem to me to answer entirely the objections Mr. d'Arcet has made to my analysis: though at the same time I am far from considering his method as a bad one. Whence then, perhaps, it will be asked, arises the difference between the results? I conceive it originates from the proportions, which Mr. d'Arcet has adopted for the carbonate of lime, and from the difficulty he must have found to deprive this salt entirely of water, without expelling some of its acid.

Causes of difference in the results.

Thenard and Biot have just made a comparative analysis of carbonate of lime and arragonite. In their experiments they employed all the care, that might be expected from such experienced chemists. From these it appeared, that 56 of lime unite with 43 of carbonic acid.

Component parts of carbonate of lime.

But Mr. d'Arcet has found, that 100 parts of crystallized subcarbonate of soda, containing 36·39 of dry subcarbonate, gave 34·81 of carbonate of lime, which, exposed to a strong fire, left 18·72 of quicklime. This quantity of lime, from the analysis above quoted, would combine with 14·37 of carbonic acid, which were accordingly contained in the 36·39 of dry subcarbonate: and my analysis would have given 13·63. If it be considered too, that I was obliged to take that analysis of Mr. d'Arcet's, in which he found most carbonate of lime, these results, I imagine, will not be thought very wide.

Calculation from these

Now if Mr. d'Arcet's experiments be calculated from my analysis of the subcarbonate of soda, it will appear, that 100 parts of the soda he analysed contain 20 of water, while I found 18·86.

brings the results very near.

* I am aware, that Mr. Berthollet junr. has detected much smaller quantities of acid with the same test. It is true, he found, that muriate of silver was soluble in concentrated and boiling solutions of almost all muriates: but it is sufficient to dilute them with water, to occasion its reappearance.

IV.

On a new Pitchlike Iron Ore, or Sulphated Iron with Excess of Base: by Mr. GILLET LAUMONT, *Correspondent of the Institute, and Member of the Council of Mines* *.

New and rare mineral from Ferber's collection,

MR. Karsten has just sent to Mr. Haüy a new and very rare species of mineral. Examining the geographical collection of the department of Mines at Berlin, that gentleman found among the Saxon minerals from Ferber's collection, which had been purchased by that department, a small box, labelled by Ferber "Pitchlike iron ore from the mine of Kustbescheerung near Freyberg."

called by him pitchlike iron ore.

Other minerals similarly named.

In his letter to Mr. Haüy Mr. Karsten observes, that mineralogists have confounded the pitchlike iron ore sometimes with the black blende of Freyburg, at other times with the oxidule of uranium, and that latterly Werner has given this name to Haüy's phosphated manganese †.

Its component parts.

Mr. Karsten quickly perceived, that this mineral did not agree with any of those known by this denomination; and his conjecture was confirmed by Klaproth, who on analysing it obtained

Oxide of iron - - - -	67
Dry sulphuric acid - - -	8
Water - - - - -	25
	100

Its characters.

The specific gravity of this mineral was 2·144. The specimens sent to Mr. Haüy are small and very brittle, but varying in colour. A piece being heated in the flame of a candle swelled up, fused pretty readily, and became magnetic.

Similar substance from a mine in Brittany.

I gave a description of a substance much resembling this in a paper on the mines of Brittany, which I presented to the Academy of Sciences in May, 1786. It was there called "an acid phosphoric martial salt." I brought it from the lead mine of Huelgoat, celebrated for the phosphates of lead, which I then made known ‡. It was among these

* Journal des Mines, vol. xxiii, p. 221.

† Jameson's Mineralogy, vol. ii, p. 569, 612. C.

‡ I obtained at that time from these lead ores four or five per cent

these lead ores, at the depth of 494 feet [527 Eng.], that this resiniform substance was found. It appeared to have been melted over several pieces, which were impregnated with it; and was commonly in very brittle, drusy masses.

I have only a few fragments of this left, and they exhibit nearly the same characters as Ferber's pitchlike iron ore, which may now be termed sulphated iron with excess of base. Dr. Weiss, of Leipsic, who was present at some experiments I lately made with the two substances, did not hesitate to consider them as the same. In fact, the substance of Huelgoat and that of Freyberg have both a deep yellow colour, varying between that of olivine and idiocrase: their aspect is equally resinous; their fracture is conchoidal, unequal, shining; their hardness superior to that of sulphate of lime, but inferior to that of carbonate of lime, which scratches them strongly, though they also scratch it slightly; their brittleness is very great; their colour, when scraped, is yellow, though that of the Freyberg mineral is a little deeper than the other.

The two compared.

Their characters.

In the flame of a candle, before the blowpipe, they swell up, and crack, assuming the colour of gamboge in the lump; and at length melt into black scoriæ, attractable by the magnet, which adhere to the platina tongs. The mineral from Freyberg only swells up more, melts more quickly, and adheres more to the tongs.

When fresh, the resiniform mineral of Huelgoat gave a white precipitate with the solution of barytes in muriatic acid, which indicated the presence of sulphuric acid; and it impressed then an acid and styptic taste on the tongue, which it has now lost: but the presence of sulphuric acid in it has been confirmed afresh by Mr. Descotils, though he had not enough to verify that of phosphoric acid.

Evidences of sulphuric acid.

cent of phosphorus, by a simple, new, and speedy method, described in that paper. I was led to it by the green flame, which I observed to rise from the crucibles in which I heated the ore. In the same way I was induced to suspect the presence of phosphorus in the resiniform ore accompanying this lead from the green flame I obtained with the blowpipe, and the white precipitate it gave with lime-water. See Journ. de Phys., May, 1786, vol. xxviii, p. 382, 385.

V.

Analysis of three Species of Pyrites, by Mr. Bucholz *.

Analysis of pyrites.

MESSRS. Gehlen and Bucholz having particular reasons for wishing to examine the observations of Proust on the two compounds of iron and sulphur, they made their experiments on an artificial sulphuret with a maximum of sulphur, because they had not any native. Having afterward procured three different specimens of native pyrites; Mr. B. determined to analyse them carefully, in consequence the difference between the results of some eminent chemists. The following are the component parts of pyrites:

Component parts according to different authors.

	Sulphur.	Iron.
according to Proust - - - -	47·36	52·64
Hatchet - - - -	53·24	46·76
Gueniveau † - - -	52·76	47·24
	53·40	46·60
	53·69	46·31
Bucholz and Gehlen -	47·93	52·07
	47·36	52·64

Usual process consumes much time and acid.

As the process generally adopted, which consists in treating pyrites with weak nitric acid, requires a great deal of acid and of time, Mr. Bucholz first endeavoured to find a process, that should convert the pyrites into oxide of iron and sulphuric acid with the least possible expenditure of both, without occasioning any loss, and without giving inaccurate results.

Process adopted to save both.

After various trials, the following method appeared to him most speedy and certain. 100 grs. of pyrites, reduced to very fine powder, were diffused in half an ounce of water in a twelve ounce phial; and concentrated nitric acid was added, drop by drop, as long as a brisk effervescence took place, with the evolution of red fumes. The mixture

* Abridged from the Annales de Chim. vol. lxviii., p. 134. Translated from the German by Berard.

† For a careful analysis by Gueniveau, where the proportions are 45 iron, 55 sulphur, see Journal, vol. xxi, p. 142. C.

being

being then exposed to a gentle heat, as soon as no farther action was produced the pyrites was completely oxided. In several trials he employed about 11 drachms of nitric acid, and the process occupied a quarter or at most half an hour. By the addition of a small quantity of water, the loss that would have been occasioned, if concentrated nitric acid had been employed, was avoided.

The first specimen analysed was a grayish yellow pyrites perfectly crystallized in cubes. The filtered solution left 4 grains of silex; and muriate of barytes threw down a precipitate amounting to 355·5 grs. Hence Mr. Bucholz calculates the pyrites to have contained 51·15 per cent of sulphur, estimating the sulphate of barytes to contain 32·5 of acid, and sulphuric acid 42·5 of sulphur. 1st specimen of pyrites.

The second specimen was a pyrites crystallized in cubes, with concave surfaces, and the edges slightly blunted. 100 grs. left 4·5 of silex, and formed 358 grs. of sulphate of barytes. This therefore, calculating on the same data, contained 51·77 per cent of sulphur. 2d specimen.

The third was a pyrites crystallized in radii 100. grains left 2 of insoluble matter, and produced 352 grs. of sulphate of barytes. Hence Mr. Bucholz calculates its sulphur at 49·61 per cent; and estimates the mean proportions at 51 sulphur, 49 iron*. 3rd. specimen. Mean.

* If we take the proportion of acid in sulphate of barytes to be 33·5 per cent, and that of sulphur in the sulphuric acid to be 43·28 per cent; the proportions which Dr. Henry has adopted in his Elements of Chemistry, lately published; we shall find, that the first specimen gave 53·66 per cent of sulphur, the 2d 54·36, and the 3d 52·66; the mean of which is 53·56 to 46·44 of iron. C.

VI. *Description*

VI.

Description of Phosphated Copper: By Mr. Hersart, *Mine Engineer**.

Essential characters.

PHOSPHATED copper, whatever be its form, is of a very dark or bottle green on its surface; but internally of a fine emerald green, bright and shining or mixed with reflections of black.

It is soluble without effervescence in nitric acid, to which it gives a sky blue colour, as it does to ammonia. Iron precipitates copper from the nitric solution.

Its specific gravity is 4·07031.

It is easily scraped with a knife, scratches pure carbonate of lime, and is scratched by common glass.

The powder is always of a lighter green than the mineral in substance.

In thin pieces it is translucid.

The fracture of its crystals is lamellar, that of the drusy specimens fibrous. The latter has not the brilliancy of the former, but in some specimens it exhibits a silky or satiny lustre.

Before the blowpipe the phosphate of copper fuses easily, producing first a brittle globule, dull, and of an ashen or blackish colour. If we continue to heat the globule on a piece of charcoal with the addition of any kind of grease, a small button of red copper will be obtained; but a part will still remain in the state of blackish scoriæ. This residuum dissolves in nitric acid with effervescence, giving it a sky-blue tinge.

If the phosphate of copper be fused before the blowpipe with borax, we obtain a bright red glass.

* Abridged from the Journal des Mines, Vol. XXIV, p. 331. Phosphate of copper not being much known to mineralogists, we imagine the following description will be found interesting, as it has just been drawn up on the spot where the mineral is found, and as it differs in some respects from the descriptions hitherto published: while, having been made from a great number of specimens, it appears to us to deserve confidence. *French Ed.*

Phosphated

Phosphated copper may be distinguished Distinguishing characters.
1, from green carbonate of copper, by dissolving in nitric acid without effervescence, and giving a blue colour:
2, from muriate of copper, by not giving a blue and green colour to flame, on which it is thrown, as the muriate does:
3, from arseniated copper, by not emitting an arsenical smell when acted on by fire, and giving a blue colour, instead of a green, to nitric acid.

The crystals of phosphated copper are commonly grouped so as to exhibit but one face, or one solid angle. The faces are seldom plane and smooth, being almost always curved, and subdivided into a great number of small facets with different inclinations. When the faces are smooth and plane, they appear as if striated parallel to one of the edges: and in these crystals we perceive two opposite faces, which are neither plane nor smooth, but rough and full of little points. The form of these crystals appears to be a rhomboid, approaching to a cube. Varieties of form.

Single or detached crystals are occasionally found. These exhibit a rhomboid apparently more acute than the preceding; but neither their faces nor edges are sharp and well defined.

Sometimes it occurs in small scales, lying on each other, and inclined in different directions.

The fibrous phosphate of copper is found either in masses externally drusy; or lining cavities. This variety exhibits internally fine and close striæ, arranged in bundles of divergent radii, issuing from one or more centres. This variety has sometimes a silky or satiny lustre within.

The phosphated copper just described is found in the bed or vein called Venusberg, or Josephberg, not above half an hour's journey from the town of Rheinbreitbach. I have said bed or vein, because, if examined in different places, it appears sometimes one, sometimes the other; and hence mineralogists are not decided which to call it. I am inclined however, to consider it as a vein, from the resemblance between this mountain and that of Mariaberg, which contains unquestionable veins of pyritous copper, and is not above 20 or 25 minutes distant from that in which the phosphated copper is found. Where found.

The

The vein of Josephberg is contained in a mountain, that appears to be composed entirely of argillaceous schist, or rather a schistose clay containing mica, but in particles scarcely perceptible.

Ores accompanying it.

The phosphate of copper accompanies copper pyrites, native copper, acicular or earthy oxidule of copper, and blue and green carbonates of copper. The latter is found also in the state of malachite, or compact carbonate of copper. Sulphate of copper too occurs, though very rarely, in this vein, which is very thick, and its extremity comes out to day near the summit of the mountain.

Gangue.

The gangue of these ores is commonly a white or grayish hyalin quartz, frequently tinged of a brown yellow by oxide of iron, which is likewise found uncombined in the specimens. Sometimes the hyalin quartz is tinged of a pale green by the phosphated copper.

Beside the hyalin quartz, but more rarely, a stone is found as its gangue, which the director of the works calls hornstein, but which I am inclined to consider as a true agate quartz. If it be rare to see this agate quartz form the mass of the specimens, it is frequently found lining their cavities. The surface of the phosphate of copper, particularly of the drusy, is often covered with a pellicle of common chalcedony, which is so thin, as not to be always perceptible; but if a fragment be exposed to the blowpipe, the chalcedony immediately separates from the phosphated copper, and appears with its white colour. This pellicle sometimes gives the druses the appearance of mouldiness, or renders their surface velvetty. In the cavities too, that contain the phosphated copper, a white chalcedony, or sometimes slightly tinged with blue, is found in separate and parallel cylindrical tubes, and occasionally in slender threads, crossing each other in various directions. These are either white or rose-coloured. In the axis of the tubular chalcedony we frequently find an opake line, apparently owing to the oxide of iron. Sometimes among the fibrous phosphate of copper particles of green carbonate of copper are found, which are distinguishable by their paler colour.

The

The preceding description differs in several respects from that of Mr. Karsten, who no doubt had but few specimens to examine. In saying, that its colour externally was grayish black, he was probably misled by some specimens covered with a pellicle of chalcedony, which in fact have this colour. Mistake of Karsten.

VII.

Comparative Analysis of Gum-Resins: By Mr. Henry Braconnot, *Professor of Natural History, &c.**

THE substances I purpose to examine are interesting, have hitherto been considered only in a few points of view, and the labours of Boulduc, Geoffroy, Neumann, and Carthcuser, leave much to be desired with respect to them. Besides, the great progress made by chemistry since their time demands a fresh examination of the gummy-resinous substances, of which modern chemists have taken but a cursory view; and this emboldens me to consider in a new light the concrete juices of vegetables, availing myself of the present state of chemical science. If I have attempted an undertaking beyond my strength, at least I will not have to reproach myself with not having done my best to merit the approbation of the learned. At present I shall bring forward only a part of my labours, intending soon to complete them. Gum-resins a fit subject of inquiry.

Article I. *Analysis of Aloes.*

§ I. The aloes, that was the subject of this examination, was of a yellowish red, and semitransparent; in its fracture it exhibited several yellow specks shining on a red ground; reduced to powder it was of a fine yellow colour; it had a very bitter taste, and a smell not disagreeable to some people. It did not become electric by friction. Aloes described.

Exposed to a heat of 80° R. [212° F.] it first softens, and then melts. This fusibility is the cause of its being much Action of heat on it.

* Abridged from Ann. de Chim. vol. LXVIII, p. 19. The paper was read to the Academy of Sciences at Nancy the 14th of Jan. 1808.

much more easily pulverable in winter than in summer. If a piece be held in the flame of a candle, it melts, swells up, and takes fire.

Distilled. § II. Fifty grammes [772 grs.] being distilled with a heat very gentle at first and incapable of decomposing the aloes, the products were:

Products. 1st, 8 gram. [123·5 grs.] of water, impregnated with the essential oil from which the smell is derived.

2d, at a higher degree of heat came over 8·7 gram. [134 grs.] of a nearly colourless water, in which I found some acetic acid, but no ammonia, on adding quicklime in powder.

3rd, 5 gram. [77·2 grs.] of a heavy red oil, soluble in alcohol.

4th, a large quantity of oily hidrogen gas and carbonic acid.

Coal. 5th, there remained in the retort, which had experienced a commencement of fusion, 20 gram. [308·8 grs.] of a hard coal, very bulky and swelled up, retaining a large quantity of hidrogen, which was seen to burn on exposing it a long time to a strong heat in a crucible for the purpose of incinerating it, which was found to be impossible. It retained all its blackness, its brilliancy, and considerable hardness; yet it had lost 12·5 gram, [193 grs.] of its weight, which I ascribed in great part to hidrogen. The 7·5 gram. [115·8 grs.] that remained contained no sensible quantity of potash.

Having treated this coal with muriatic acid, the filtered liquor was precipitated by ammonia, which separated some oxide of iron, and a small quantity of phosphate of lime. Carbonate of potash precipitated a few decigrammes of carbonate of lime.

On heating nitric acid on this coal a small quantity of tannin is obtained, which precipitates glue.

Aloes completely dissolved in a large quantity of cold water, § III. Powdered aloes, triturated in a glass mortar with cold water, yielded a substance having the tenacity of turpentine when worked between the hands. I obtained a complete solution by successive additions of water, but it required a large quantity. The last portion, that remained to be dissolved, was similar to the first in its bitterness and other properties. This solution froths when shaken.

148 gram.

148 grains. [2285 grs.] of water, at 32° R. [104° F.] were sufficient for the complete solution of 4 gram. [61·8 grs.] of aloes, except a decigr. [1·5 gr.] of impure woody matter. As it cooled, the solution grew turbid, and let fall a portion of the matter dissolved. This solubility of aloes in water is so much increased by heat, that we may obtain a solution of the consistence of a sirup, which then lets fall nothing, and is even capable of crystallizing, on boiling it down still farther. but much more soluble in hot.

The aqueous solution of aloes exhibited the following appearances with reagents. Properties of the aqueous solution.

1. Infusion of litmus is very perceptibly reddened by it.

2. Alkalis and lime-water render its colour deeper, without precipitating any thing.

3. Sulphate of iron produces in it a brown colour, and in a little time a precipitate of the same hue.

4. Decoction of galls produces a yellowish flocculent precipitate. The supernatant liquid is paler, and much less bitter than before.

5. The subacetate of lead likewise occasions a precipitate, and the supernatant liquid is nearly colourless.

5. Nitrate of copper and of lead, and muriate of tin, likewise occasion slight depositions; but these do not appear to be real chemical compounds, for the solutions of muriate of soda, and other neutral salts, effect as much. These saline substances therefore act on the solution of aloes in the same manner as on that of tannin in water, merely by weakening the action of this liquid on the difficultly soluble matter it contains.

This solution of aloes, which was of a fine gold colour, was left to settle in three vessels. The first, containing a quart, was quite filled with it, and well-corked: the second, of the same size, was but half full, and uncorked: the third, an apothecary's phial, was likewise open, and but a quarter filled. At the end of ten weeks the solution in the first retained its colour unchanged: that in the second was of a very deep red, but rendered colourless by oximuriatic acid, which produced in it a flocculent precipitate: in the third, a quantity of mucus was formed. The coloured liquids in the last two had acquired a degree of viscosity: Action of air on the solution.

cosity: in fact, a matter analagous to gelatine seemed to be formed, for decoction of galls produced in them a precipitate much more copious than in the recent solution.

These facts seem to me to prove, that aloes is not a resin.

Solution of aloes in alcohol.

§ IV. Spirit of wine, at 38° [sp. grav. 0·827] dissolves aloes entirely with great readiness, particularly if hot, which indicates the absence of gummy or extractive matter. The solution, when filtered to free it from some particles of foreign matter, is of so deep a red, that it is difficult to perceive its transparency. Water produces in it a copious sediment of a pale yellow colour. This colour is owing to the water retained in it, for on drying, it assumes its original brown.

Crystallizable.

If the alcoholic solution be evaporated, towards the end we find, that the least motion, the slightest breath on it, produces a sort of crystallization, which disappears again, but is soon after reproduced.

Aloes insoluble in oils.

Though alcohol dissolves this substance very well, it is not the same with oil, either fixed or volatile. I exposed to heat a mixture of olive oil and aloes: the latter remained fused at the bottom. Oil of turpentine boiled on aloes comported itself nearly in the same manner, but it acquired a light amber tinge.

Action of alkalis on aloes.

§ V. Alkaline solutions dissolve aloes very readily without heat, and the results are combinations, in which the bitterness appears partly concealed *. Acids throw down from these solutions copious precipitates, which become coloured by desiccation. Volatile alkali diluted with water likewise dissolves aloes completely. The filtered solution was of a deep red, and was evaporated slowly, to expel the excess of ammonia. As the solution was concentrating, a continual movement appeared on the surface, seeming to indicate a tendency to crystallization, for needles were observed appearing and disappearing in succession. On continuing the evaporation almost to dryness, needly crystals were obtained, imbedded in a kind of resiniform mass. On

* The mixture of seven drachms of tincture of aloes with one drachm of the liquid subcarbonate of potash has the taste of a solution of extract of liquorice very nearly. C.

heating

heating this with a certain quantity of lime and water, ammonia was very perceptibly evolved.

§ VI. Weak acids have no very striking action on aloes, yet they dissolve it better than water, which whitens the solution of aloes in distilled vinegar. The mineral acids act on it much more powerfully. Nitric acid dissolves it very well without heat, producing a deep red liquid, which water precipitates abundantly. Action of acid on aloes.

Ten gram. [154·4 grs.] of aloes were treated in a retort with 80 gram. [1235 grs. of nitric acid at 36°, taking care to raise the fire cautiously. The action was brisk, and abundance of red fumes were evolved. When these disappeared, the retort was removed from the fire. The liquid contained in it was of a deep yellow colour, and on cooling deposited a pretty large quantity of a yellow flocculent substance. Being evaporated to the consistence of honey, it was diluted with water, and filtered. A yellow substance remained on the filter, which, after having been well washed and dried, amounted to about a fourth of the aloes employed. This appeared to be an acid, analogous to the yellow, acid, and detonating matter which Fourcroy and Vauquelin obtained by the action of nitric acid and animal substances, but differing slightly in several respects. Nitric acid. Yellow acid produced.

The yellow aloetic acid, well washed and dried, is of a fine yellow colour, and extremely bitter. It does not crystallize. It reddens litmus paper, and effervesces with alkaline carbonates. It has a pleasant aromatic smell, particularly when gently heated. It melts like nitre, emits an aromatic vapour mixed with bitterness, and leaves an abundant coally residuum. Distilled with a gentle heat, it furnished all the products of vegetable substances, and finally detonated with a purplish flame. A very bulky coal remained, equal to a third of the matter employed. Its properties.

This acid is very little soluble in water. It required 1250 times its weight of water at 10° R. [54·5 F.] for its complete solution. This was of the fine red colour of arterial blood. Muriate of tin produced in it a precipitate of the colour of wine lees. The sulphates of iron and of copper brightened the colour.

Alcohol

Alcohol at 38° dissolved only one thirtieth its weight of the yellow acid. The solution was a very deep red.

Hot mineral acids dissolve this yellow matter without evolving any thing; but it is soon after deposited, in consequence of its insolubility.

Neutralized with potash detonates.

Potash forms with it a compound of a deep red, and capable of crystallizing. This red salt detonates with the violence of gunpowder, either when exposed to a certain degree of heat, or touched with a burning coal; and after burning leaves a slight coally trace, and a remarkable smell of prussic acid, which leads to a suspicion of the presence of nitrogen.

This red detonating substance is easily produced by pouring on the yellow acid of aloes a weak hot solution of caustic potash, which has but a slight solvent action on it.

Nitric solution.

The nitric solution, from which the yellow aloetic acid has been separated, was saturated by potash. At the end of twenty-four hours a very small quantity of red detonating matter was deposited. Nitrate of lime being added to it, a copious precipitate of oxalite of lime took place, which, when well washed and dried, weighed 3·5 gram. [54 grs.] The liquid separated from the oxalate of lime was precipitated by nitrate of lead; and the precipitate, treated with a third of its weight of dilute sulphuric acid, yielded about a gramme [15·4 grs.] of malic acid partly dried.

Not a gum-resin,

§ VII. From these facts it follows, that aloes is not a gum-resin, as has been supposed, since neither of these two principles is found combined in it. Consequently too it cannot be confounded with the resins, though it is more similar to them than to the gums. It is therefore a principle

but a simple substance,

sui generis, which from its properties I would call *resinoumer*. This immediate principle is probably very common, and has its species, like other vegetable matters.

occurring in other plants.

It is this, that was at first confounded with resins, that has been sometimes taken for oxigenated extract, and that Mr. Vauquelin has made known in his interesting paper on the different species of cinchona*. It is the same substance too, that is deposited in greater or less quantity from the

* See Journal, vol. xix, p. 106, 203.

decoctions of several bitter plants of the class syngenesia, in which febrifuge virtues have long been acknowledged, as wormwood, centaurea calcitrapa and benedicta, succory, dandelion, and likewise fumitory *. It is true, that these plants have been found less efficacious than the astringent febrifuges; and I am persuaded, that the principle in cinchona, which acts specifically against fever, and the periodicalness of diseases, is owing to the combination of the resinoamer with tannin, or a similar matter. Following these ideas, my colleague, Dr. Haldat, intends to make some important experiments, that may lead to great and useful discoveries, and of which he will give an account.

We know that aloes, taken internally, is a very active tonic, and externally is a very powerful antiseptic. Would it have this antiseptic power internally? It is likewise acknowledged, to have febrifuge and purgative properties: but it is not commonly known, that it ceases to purge when mixed with powdered galls, a fact I have found by experience. Medical properties of aloes. Galls destroy its purgative properties.

REMARKS.

TO complete this examination of a valuable drug, pretty extensively used in physic, before we proceed with prof. Braconnot to other gum-resins, we shall give an abstract of the analyses of it by Trommsdorff and by Bouillon-Lagrange and Vogel, both in the same volume of the Ann. de Chimie, that by Trommsdorff being taken from his Journal of Pharmacy. The following are the results of Mr. Trommsdorff's analysis. Other analyses of aloes.

1. *Succotrine aloes* dissolves entirely in boiling water; but the resinous part separates on cooling. Results of Trommsdorff's analysis.

2. It dissolves also in alcohol without leaving any residuum.

3. The parts soluble in water contain more bitter principle than those soluble in alcohol, though the latter are not destitute of it.

* It appears to me, that the resiniform matter found in bile by Thenard has a great deal of similarity with the resinoamer of aloes.

4. The

4. The *hepatic* differs from the succotrine aloes in containing some albuminous animal matter, and less resin.

5. It does not dissolve completely in boiling water, because the heat coagulates the albumen.

6. Neither is it totally soluble in alcohol. This readily distinguishes it from succotrine aloes.

7. The saponaceous principle* and resin appear to be of the same nature in both kinds.

Component parts of succotrine and hepatic aloes.

8. Succotrine aloes consists of 75 parts of bitter saponaceous principle, 25 parts resin, and a trace of gallic acid.

9. Hepatic aloes contain 81·25 saponaceous principle, 6·25 of resin, 12·5 of albumen, and a trace of gallic acid.

Bouillon-Lagrange and Vogel's analysis by dry distillation,

Messrs. Lagrange and Vogel experimented on much larger quantities than either Braconnot or Trommsdorff. They distilled a kilogramme [near 2¼ lbs avoird.] of each kind in a large glass retort. Toward the end of the process a shining black substance sublimed, which was nothing but aloes. The water from the dry distillation of the hepatic aloes they say was perceptibly ammoniacal; that from the succotrine merely exhibited a white vapour with muriatic acid, after the addition of a little pure potash.

and by wet.

They afterward distilled a similar quantity of each, previously diluted with a quart of water. The succotrine aloes

Succotrine aloes.

yielded a liquid not acid, of a very sweet and pleasing smell, on which floated a volatile oil of a greenish yellow colour, and smelling like that of melilot. It also contained some other substance, as Trommsdorff observed, for after some time it grew cloudy.

Hepatic.

The water from the hepatic aloes was not pleasing to the smell, but rather nauseous, approaching a little to that of prussic acid. There was no trace of oil on its surface or in solution.

Saponaceous principle of plants.

* The author having dissolved a portion of aloes by boiling in 12 parts of water, a fourth part of the aloes separated on cooling. The aqueous solution being evaporated to dryness, a bitter substance resembling aloes remained, which was completely soluble in alcohol, but altogether insoluble in ether. Hence he supposes it to be the saponaceous principle of Hermbstaedt, *seifenstoff*, or *pflanzenseif*, which is thus characterized, and occurs in various vegetables, as saffron, rhubarb, &c. He supposes there are different species of it, more or less bitter to the taste.

They

They did not find aloes soluble in cold water like Mr. Braconnot. On a quantity of succotrine aloes in powder they poured water at 8° R. [50°F], assisting its action by frequent stirring. The clear supernatant liquor, after settling, was decanted off, and another quantity of water poured on the residuum. This was repeated, till the water, after standing on the residuum four and twenty hours, was found destitute both of taste and colour. The glutinous matter remaining was then worked between the fingers under a stream of water. Only in part dissolved by cold water.

The first liquor poured off was very brown, and strongly impregnated with the aloes; the second and third were much less so, the rest growing weaker in succession, till the last was clear water. When the aloes had been sufficiently washed, and thus exhausted by water at 8° [50°F.], there remained a soft grayish mass, very elastic, which, when wet with water, did not stick to the fingers. Insoluble part.

The aqueous solution of aloes, as Trommsdorff observed, evaporated gently to dryness, leaves a substance soluble in water and alcohol, but scarcely at all in ether. The resinous matter of aloes, on the contrary, is soluble in alcohol and in ether, but not in water at 10° [54·5°F.] The former dissolves readily in cold nitric acid at 36°, and forms a green liquid, which is scarcely rendered turbid on the addition of a little water, and becomes perfectly clear when farther diluted. The resinous part is more difficultly acted on by this acid, and produces a red solution, which, though much weaker than the former, throws down a resinous, sticky, insoluble substance, on the addition of a little water. Two different substances in aloes.

Nitric acid heated on aloes produced a fine yellow powder, and nearly the same phenomena as those observed by Mr. Braconnot. This powder, diffused in a little water, communicated to it a superb purple, very rich in colour. A single atom will tinge a very large quantity of water. This colour is so permanent, that the skin remains dyed with it for several days, particularly if an alkaline salifiable base have been previously added to the powder. Action of nitric acid. Fine purple dye.

Messrs. B. L. and V. likewise passed a current of oxi-muriatic acid gas into a concentrated solution of aloes in Action of oxi-muriatic acid

gas on the part soluble in water

cold water. A large quantity of the gas was absorbed, and the solution became yellow, and coagulated like animal jelly, so as to become almost one entire mass. The coagulum, when separated, was of a whitish yellow, but soon turned brown. After being washed, it was very elastic, insoluble in water at 8° R. [50°F], but very readily soluble in alcohol, and this solution was copiously precipitated by water. The oximuriatic acid gas therefore appears in some sort to have resinified the portion of aloes soluble in water.

converted it into a kind of resin.

Component parts of aloes.

According to them, succotrine aloes consists of 68 parts extract, and 32 resin: and hepatic aloes is composed of 52 extract, 42 resin, and 6 insoluble matter, which Trommsdorff calls albumen.

(To be continued.)

VIII.

Communications concerning the Royal Botanical Garden at St. Vincent, from its Superintendant Dr. Alexander Anderson, *to* Dr. C. Taylor *.

Dear Sir,

I Am honoured with your letter of the 26th and 28th of April, with the 21st and 24th volumes of the Transactions of the Society of Arts; also the publication on the Culture of Black Pepper, for which I feel great obligation to the Society.

Cultivation of the pepper plant at St. Vincent's.

From Mr. Martyn's account of the pepper plant, I am in hopes that it will succeed in this garden; as he says it is three or four years before they produce in the East-Indies after planting, and it is now near that time since I procured them, and there are several very luxuriant at present †. I am

* Trans. of the Soc. of Arts, vol. xxvi, p. 234.

Success of the black pepper plant in the West Indies.

† In a subsequent letter, dated June the 19th, 1809, the Dr. says: "I have the pleasure to inform the Society, that the black pepper plant thrives remarkably well in this garden, and has been producing fruit more than a year. Some of its produce I now transmit to

am happy to find I had adopted the mode of planting them which he has described.

In general I find that East-India plants are more rapid in their growth, either from seeds or plants, than the indigenous plants of the country, and arrive at perfection sooner; but the reverse is the case with the Chinese. There is at present in the garden a large tree of the *litche*, sent by sir Joseph Banks in 1788, which as yet has made no attempt to flower. I experience the same disposition in several herbaceous perennial plants from China. I was pleased to see a specification of growth of trees in the East Indies, by Dr. Roxburgh, in the last volume of the Transactions, which led me to a comparison of some East Indian trees here, and also of some natives; and I find those from India thrive full as well here as in their native soil. The result I send you. It is a matter of curiosity, rather than utility. It shows the rapid progress of vegetation in tropical climates, compared with that in the colder regions.

In the West Indies East India plants thrive, Chinese do not.

Of the numberless articles for commerce and economy, manufactured in the East-Indies, no attention is paid to them here, although many of them are common. The same is the case as to small products for necessary existence. This is owing to the want of a proper population, and the high price of manual labour. Except in Barbadoes, and a few other islands, all the land in cultivation is engrossed by the sugar cane. No room is left for poor industrious people, unless in detached spots remote from towns, markets, and shipping. The hard woods fit for mill timber are more attended to than any other, and they undoubtedly are the most essential article to the planters, yet few take the trouble to plant them, or give room for them.

Many natural productions neglected for the sugarcane.

You mention the high price of oak bark for tanning. I am confident we have many barks here superior to it, as to the astringent principle. Whether our barks are as effectual, or more so, than the oak bark in tanning, deserves ex-

Tan to be had in the West Indies.

to you for the Society's inspection. The berries are collected before full maturity. I find it is a plant of more easy cultivation than I conjectured. After it begins to bear there is no intermission. It yields its berries in succession during the year. As soon as one crop comes to maturity, the plant recommences flowering.

periment. For that end I will transmit you some specimens by the first opportunity. The barks might be imported at a trifling expence, unless the customhouse duties should be found to prevent them. The high duties and prohibitions in the customhouse prevent several people here from sending similar articles hence, for experiments, as well as for speculation in trade. A few persons in this island wish to cultivate the cinnamon for commerce; they have asked me if it can be entered at the customhouse, and what are the duties upon it? I could give them no information as to either. The overhauling and pilfering, by the customhouse officers in England, of articles of natural history, sent as specimens, is very injurious; such things should be held sacred.

Obstacles at the customhouse.

Many articles here deserve to be subjected to experiments, from which I am prevented by the necessary attentions to the garden, particularly for some time past. The business of it engrosses all my time and care, and is as much as one individual can attend to.

I will endeavour to obtain a correspondence with Dr. Roxburgh, but I almost despair of it during the war. I have correspondents in America, whom I can depend upon; but the conveyance, through the medium of American vessels to these islands, is very precarious. Some time ago I lost a parcel of seeds from New-York, sent in charge by one of these vessels. I believe I mentioned, that I have lost one of my nutmeg plants, for which I blame myself, by too much attention in watering it in dry weather. The other thrives remarkably well, and is now above ten feet high; but if it proves a male, I am at a stand. Could I find the opportunity of sending by a flag of truce to Cayenne, I know I could get a supply.

Correspondence interrupted by the war.

Nutmeg killed by overwatering.

I am, with most sincere regard,
Dear Sir,
Your much obliged and ever grateful Servant,
ALEXANDER ANDERSON.

Botanical Garden, St. Vincent, July 21, 1807.

DEAR SIR,

I Have the pleasure to inform you, that some of the black pepper plants are now pushing out freely their fructification; but have to lament, that the only nutmeg in

Black pepper.

Nutmeg.

in the garden proves a male, and there is no prospect, at present, of obtaining more, as in the present situation of affairs, no communication from St. Vincent to Cayenne can be had by flags of truce or otherwise. Several plants of it were brought to Trinidad with the colony of the Chinese: I much fear they are, or will be lost there. Chinese colony at Trinadad.

I send you some cloves, about two thirds of the produce of one young tree for the first time. My reason for troubling the Society with them is from a wish to know whether drying them in the shade or sun is the most proper mode, or if it makes any difference in the quality of the spice; if not, they may be cured in the sun with no trouble, in a very short time. The young fruit I reserved on the tree for seed, part of which was beaten off by the wind, and seems to me little inferior to the flower buds. Cloves.

On reading, in the Society's Transactions, Dr. Roxburgh's Experiments on the Comparative Qualities of Bark of East India Plants as Substitutes for European Hemp*, I was induced to try the leaves of the *agave*, to ascertain how far the fibres of them would answer the purpose. I transmit a specimen of them for the society's inspection. The small bundle, tied with some twine made of the same, is the produce of one moderate sized leaf, and was obtained from it, immediately cut from the plant, in a very short time. The operation was performed by a black boy. The plants are produced in abundance by nature among the rocks by the seaside and barren hills. If found useful, any quantity may be obtained with little labour and no expense of first cost. The superior advantage over the East India articles (most of them common in these islands) is the trifling labour requisite to get the fibres from the fleshy substance of the leaves without steeping, or any other previous process. When macerated in water, I think it lessens the strength of the fibre, and gives it a dusky hue. Substitutes for hemp. Fibres of the aloe.

The three small bundles, which I now send, were taken from two species common in St. Vincent, viz. *agave vivipara*, and *a. cubensis*. The leaves of all the tropical Much valued by the Mexicans.

* See Journal, vol. xi, p. 32, and xvi, p. 223.

species

species possess much the same properties. By the ancient Mexicans, the agave was deemed the most valuable production of nature. It is mentioned by all the Spanish writers on America under the name *maguei*.

Maguei of the Spaniards.

Nothing attended to but sugar.

It is to be observed, that no article in these islands, however valuable, and whatever encouragement may be held out for its manufacture, will be attended to in their present situation. The sugar cane is considered as the only plant, that merits the attention of the planters.

Substitutes for oak bark.

In my last to you I mentioned barks of trees in these islands, which I conceived may become substitutes for oak bark in tanning. I transmit you specimens from five different trees, which are all common, and consequently readily procured, if they prove useful. That of the *maljughra* I know the Spaniards use on the main land with that intention. The quantity of each is purposely small, for the more easy conveyance, and prevention of difficulties at the customhouse. However, they may be sufficient for ascertaining their astringent or tanning principle.

In consequence of the war cutting off most of my opportunities of correspondence, the additions to the garden are much less, than otherwise they would have been; however, almost every day some thing or other is obtained from some part of the world. What I have long wished for, the grains of Paradise, are thriving luxuriantly. By the last fleet a number of East India seeds arrived; many of them will be valuable acquisitions, if they vegetate.

Grains of Paradise.

East India seeds.

I am, with the greatest regard,

Dear Sir,

Your much obliged,

Most humble and obedient Servant,

ALEXANDER ANDERSON.

St. Vincent, Botanical Garden,
April 16, 1808.

Table

Table of the Growth of certain Trees in the Botanical Garden at St. Vincent.

Growth of trees in the botanical garden at St. Vincent's.

Tectona grandis—The seeds lie in the ground from eighteen months to two years, before they vegetate. They have produced seeds in the garden ten years ago.

			ft.	in.
First seeds received from Sir J. Banks in	1788			
Circumference of stem, in	1807	at 6 ft. above ground	4	6
Caryotà urena, seeds from Sir J. Banks,	1792	Do.	4	10
Sapindus edulis, (Litche) plant from ditto	1788	Do.	4	8
Mimosa Lebbeck, seeds do.	1792	Do.	4	5
Sterculia fœtida, do. do.	1792	Do.	6	0
Gomutu Palm, seeds from Bd. of Agri.	1800	Do.	5	7
Artocarpus incisus, small plants -	1793	Do.	6	1
integrifolius, do. -	1793	Do.	5	6
Jambolifera pedunculata, do. -	1793	Do.	5	9½
Aleurites triloba, seeds - -	1793	Do.	4	8
Eugenia Malaccensis, small plants -	1793	Do.	3	10½
Mangifera indica, from seeds -	1788	Do.	7	0
Ditto, small plants from E. I. -	1793	Do.	5	2
NATIVES.				
Swietenia Mahagoni, seeds has been producing plenty of seeds for several years	1790	Do.	8	4
Copifra officinalis, seeds from the Continent	1790	Do.	3	2
One of the most valuable woods.				
Mimosa grandis, seedling plant from the Continent	1792	Do.	6	6
A very hard and valuable wood.				
Carolinia insignis, seeds from Trinidad	1787	Do.	8	0
The wood of no value.				

St. Vincent, July 21, 1807.

A. ANDERSON.

IX.

On the Oxides of Iron. By THOMAS THOMSON, *M. D. F. R. S. E. Fellow of the Imper. Chudrurgo-Med. Acad. of Petersburg.*

Remarks on Dr. Thomson's Chemistry by Hassenfratz.

IN the Annales de Chimie for May 1809 (vol. lxx, p. 145) there is an article by Mr. Hassenfratz, of which the following is an abstract.

"I have

"I have just received a copy of Thomson's System of Chemistry translated by Riffaut. I opened the first volume, and read with eagerness the tenth section, which treats of iron. The details published in that section were the more interesting to me, as I have been for these two years employed by the minister of the Interior, to describe the art of extracting iron from its ores, and to explain the different operations, which it undergoes before it is brought into the commercial world in the states of cast iron, iron, and steel. You may guess my astonishment, when I read the following passage. '*The peroxide of iron is also found native in great abundance. Proust proved it to be composed of* 48 *parts of oxigen and* 52 *of iron. Consequently the protoxide, when converted into red oxide absorbs* 0·40 *of oxigen; or, which is the same thing, the red oxide is composed of* 66·5 *parts of black oxide, and* 33·5 *parts of oxigen. One hundred parts of iron, when converted into a protoxide, absorb* 37 *parts of oxigen, and the oxide weighs* 137; *when converted into peroxide, it absorbs* 55 *additional parts of oxigen, and the oxide weighs* 192·3.

Proust's account of the oxides of iron.

"Proust has not said, in any work that I know, that the red oxide is composed of 48 parts of oxigen and 52 of iron. What may have led Dr. Thomson into errour is, that in the memoir of the celebrated chemist of Madrid, published in vol. xxiii, p. 85, of the *Annales de Chimie*, it is stated, that he announces the existence of the two oxides of iron, the one at $\frac{27}{100}$ of oxigen, the other at $\frac{48}{100}$. As it is not said in any article of the memoir, whether the 48 of oxigen were in the 100 of oxide, or combined with 100 of metal, this manner of expressing the proportion of oxigen has left a kind of uncertainty in the minds of those chemists, who have made no experiments on the proportion of oxigen in the oxide of iron. The learned British chemist, who certainly has made no experiment to resolve the question, has adopted the simplest meaning of the fraction $\frac{48}{100}$; and this has occasioned the errour in the passage, which I have quoted;"

Usual meaning of his fractional expression.

Mr. Hassenfratz then proceeds to show, that in other parts of his writings Proust is in the habit of denoting by the numerator of his fraction the quantity of oxigen, and by

by the denominator the quantity of metal: of course $\frac{48}{100}$ mean an oxide composed of 100 iron and 48 oxigen. He then proceeds to point out the true composition of the oxides of iron, and thus to correct the above passage in my work. But it is not necessary to transcribe the rest of his paper, as he had already published an elaborate dissertation on the subject in the lxix volume of the *Annales de Chimie*, in which the subject is much more fully discussed; and to which therefore I refer the reader *.

Experiments instituted to ascertain the true proportions in oxides of iron.

The perusal of Mr. Hassenfratz's paper, while it convinced me of the mistake into which I had fallen, induced me to make some experiments on the composition of the oxides of iron, in order to verify and establish the proportions obtained by others. My object at present is to state the results which I obtained.

Red oxide.

I. The red oxide of iron, or the oxide containing a maximum of oxigen, is too well known to require a particular description here. Two methods have been followed by chemists, to ascertain the proportion of oxigen which it contains. The first is to expose a determinate weight of iron to a red heat, triturating it occasionally, till it ceases to acquire any additional weight. The second is to dissolve iron in acids, and to expose the salt obtained to a heat sufficiently high to decompose it. The red oxide remains, and its weight gives the addition, which the iron has acquired by its oxidizement.

Two methods employed.

Iron filings calcined.

The first method appears at first sight easy, but it is in reality exceedingly difficult. Accordingly the experiments of Scheffer, Morveau, Lavoisier, Darso, Bucholz, and Hassenfratz differ so much from each other, that no satisfactory conclusion can be drawn from them. I consider the experiment of Hassenfratz as the most accurate. 100 parts of iron in his trial were converted into 145 of red oxide †. In Darso's experiment 100 parts of iron were augmented to 156 of red oxide ‡. But as this greatly ex-

* A translation of this paper is intended for insertion in this Journal at an early opportunity. A shorter paper of Hassenfratz on the same subject occurs in vol. xxvi, p. 47. C.

† Ann. de Chim. vol. lxvii, p. 309. Journal, vol. xxvi, p. 147.

‡ Journal de Phys. 1809 tom. ii, p. 294. Journal vol. xvii, p. 224.

ceeds

ceeds what was obtained by every other person, we must suppose a mistake. I have not tried this method, being deterred by its uncertainty.

Difficult to find iron perfectly pure.

The second method is easier, and more satisfactory. The greatest difficulty, to which it is liable, is that of procuring iron in a state of absolute purity, to make experiments upon. I have tried many varieties, and have applied to those artists, who were likely to have iron in the greatest purity. But hitherto I have not been lucky enough to find a single specimen absolutely pure. I was obliged therefore to analyse the specimens which I employed, and to make allowance for the impurities, which varied in different specimens from $\frac{1}{100}$th to $\frac{1}{1000}$th part of the whole. Polished iron wire is most convenient. Iron filings, unless made on purpose, are not sufficiently pure, and it is more difficult to dissolve them completely than iron wire.

Specimens analysed.

Polished iron wire best.

Dissolved in nitric acid.

100 grains of iron wire were dissolved in diluted nitric acid. The solution goes on rapidly, and is at first opake, and almost black, owing to the nitrous gas which it retains. This gas gradually separates, and then the liquid is nearly colourless. When concentrated it becomes of a brownish yellow colour. It was evaporated to dryness, and exposed for a quarter of an hour to a red heat in a platinum crucible. The red oxide thus obtained weighed 142·6 grains. In another experiment made in the same way 100 grains of iron were converted into 144·75 of red oxide. This last result I consider as the most correct, because it coincides nearly with the result obtained by Hassenfratz in a different manner, and because in experiments of this nature, where liquids are evaporated to dryness, there is always a risk of some loss during the evaporation. On this account, in making choice of various results, that which gives the greatest weight has the most chance of being correct. Upon the whole then we may conclude with considerable probability, that the red oxide of iron is composed of 100 iron and 45 oxigen.

Reduced to red oxide

gained near 45 per cent.

It could not be deoxidated by heat alone,

I tried to deprive the red oxide of iron of part of its oxigen by various methods, but without success. No degree of heat, which I could raise, was capable of disengaging oxigen gas from it, though the oxide acquired a black colour

colour. When the red oxide is mixed with oil, and heated to redness, it becomes black, and is attracted by the magnet: but its weight is not altered. Indeed, if we repeat the experiment a great number of times with the same portion of oxide, the weight rather increases. When red oxide is heated with charcoal, it is reduced to the metallic state.

but with the addition of oil became magnetic; though without any alteration in weight. With charcoal reduced.

When iron is dissolved in sulphuric acid, the solution evaporated to dryness and exposed to a strong heat, the sulphuric acid is dissipated, and red oxide of iron obtained. But experiments made in this way do not lead to a satisfactory result. 100 parts of iron thus treated were converted into 150 parts of red oxide. But it was not quite pure, still containing traces of sulphuric acid. This was the case even when the oxide had been exposed to a heat sufficient to calcine carbonate of lime. The results were not more satisfactory, when the iron was precipitated from sulphuric acid by an alkali. The oxide obtained, though carefully edulcorated, still contained sulphuric acid. For when dissolved in muriatic acid, and mixed with muriate of barytes, a white insoluble precipitate fell.

Iron dissolved in sulphuric acid.

Results unsatisfactory from the retention of acid.

II. To ascertain the proportion of oxigen in the black oxide of iron is a more difficult task. I shall relate the experiments which I made in order to determine the point.

Difficult to ascertain the proportions of black oxide.

1. When 100 grains of iron are dissolved in diluted sulphuric acid, the hidrogen gas produced amounts to 163·4 cubic inches, at the temperature of 60°, and when the barometer stands at 30 inches. Two experiments were made, each of which gave exactly the same result. Now it is well known, that when iron is dissolved in this manner, it is converted into black oxide. Water is decomposed, the hidrogen of which escapes in the form of gas, while the oxigen unites with the iron. It has been established, that the constituents of water, reduced to the gaseous state, consist of 2 parts by bulk of hidrogen and 1 part of oxigen. Hence in this case the oxigen, which combined with the 100 grains of iron, and converted it into black oxide, is equivalent to 81·7 cubic inches. Now 81·7 cubic inches of oxigen gas weigh, according to the experiments of Lavoisier and Davy, 27·93 grains; according to those of Allen and

Iron dissolved in dilute sulphuric acid.

Pepys 27·63 grains. The above experiments of mine were made at the temperature of 45°. If the vapour of water be subtracted according to Mr. Dalton's formula, it will diminish the weight of the oxigen about one third of a grain. It follows pretty nearly from these data, that black oxide of iron is composed of 100 parts by weight of iron and 27·5 of oxigen. Bergman, Berthollet, Vandermonde, and Monge made many experiments on the quantity of hidrogen gas given out, when iron is dissolved in diluted sulphuric acid; but their results differ so much among themselves, owing probably to the great difference in the purity of the different specimens of iron employed, that no satisfactory consequences can be deduced from them.

Black oxide contains 27·5 of oxigen to 100 metal.

2. When iron wire is burnt in oxigen gas, it is converted into black oxide. Mr. Lavoisier made many experiments on this combustion, from which he concluded, that 100 parts of iron combine with between 32 and 35 parts of oxigen*. I repeated this experiment several times, with every possible precaution to insure accuracy. All the trials corresponded so nearly, that it will be only necessary to state one of them. 11·81 grains of iron wire were burnt in oxigen gas. The black oxide formed weighed 15·01 grains. Hence 100 parts of iron would by this process have been converted into 127·09 grains. This result agrees nearly with the preceding. The proportion of oxigen, which appears to combine with the iron, is indeed a little lower. But the reason I believe to be, that, during the combustion of the iron, small particles of it are dissipated in sparks, which cannot afterward be collected and weighed. This quantity is indeed very minute; but still it is something, and may be seen very well, when we examine the cloth upon which the oxide is washed. If it amounted in the preceding experiment to the 20th part of a grain, it would bring up the proportion of oxigen to 27·5, the same which was deduced from the hidrogen gas emitted during the solution of iron in diluted sulphuric acid.

Iron wire burned in oxigen gas.

The result agreed with the preceding.

3. When black oxide of iron is dissolved in nitric acid, the solution evaporated to dryness, and the dry mass exposed

The black oxide dissolved in

* Annales de Chimie, tom. I, 19.

to

to a red heat, it is converted into red oxide. This furnishes us with another method of estimating the quantity of oxigen in black oxide of iron. Bucholz had recourse to it, and found, that 100 parts of black oxide are by this treatment converted into 110 of red oxide*. On repeating the experiment, I found it attended with more difficulty than I expected. It is not easy to procure black oxide in a state of purity. My first trials differed so much from each other, that I was obliged to conclude, that my black oxide contained some red oxide mixed with it. Another difficulty is to dissolve black oxide of iron in nitric acid. It resists the action of that acid with great obstinacy, even when in the state of a fine powder. After repeated failures, I at last succceded in obtaining results, which agreed with each other. The following I consider as the most accurate of these. 16·77 grains of pure black oxide were dissolved in nitric acid. The solution was evaporated to dryness, and the dry mass exposed to a red heat, in a platinum crucible. It weighed 19·1 grains. Hence 100 grains of black oxide by this treatment would have been converted into 113·89 grains of red oxide. Now if red oxide be a compound of 100 metal and 45 oxigen, it is obvious, that 113·89 grains of red oxide contain 78·5 grains of metal, therefore 100 parts of black oxide are composed of 78·5 metal and 21·5 oxigen, or the oxide consists of 100 metal combined with 27 oxigen—a result which agrees very nearly with that deduced from the two proceeding sets of experiments.

nitric acid and converted into red.

This experiment difficult.

The most accurate

gave similar results with the preceding.

4. I introduced 300 grains of polished iron wire into a porcelain tube, placed the tube in a furnace horizontally, heated it to redness, and then caused a current of steam to pass through it for several hours. By this process it is well known that the iron is converted into black oxide, while hidrogen gas is evolved in abundance. The evolution of this gas is accounted for by the decomposition of the steam. The oxigen is conceived to unite with the iron, while the hidrogen passes off in the form of gas. By this method I expected to be able to ascertain directly the increase of weight, which takes place when iron is converted into black oxide. But I was disappointed. Though the experiments

Iron wire converted into black oxide by steam.

Results unsatisfactory.

* Journal, vol. xxv, p. 354.

were

were made with great care, they presented anomalies, which it was impossible to reconcile with the opinions at present received. I shall describe one of my experiments particularly. Of the 300 grains of iron introduced, 63·37 grains were still found in the state of iron at the end of the experiment. The surface indeed had lost its lustre, but the malleability and other qualities remained. The specific gravity of the black oxide formed was 5·025, which agrees nearly with that of specular iron ore. The hidrogen gas evolved, reduced to the temp. of 60°, barometer 30 inches, measured 415·5 cubic inches. Hence the oxigen, which combined with the 236·63 grains of iron that had been converted into black oxide, must have been equivalent to 207·75 cubic inches, or 69 grains nearly. But if 236·63 grains of iron combine with 69 grains of oxigen to be converted into black oxide, it is obvious, that 100 grains would have combined with 29·1 grains of oxigen. This is a greater proportion than results from the preceding experiments; but the apparent differences was probably owing to the surface of the wire, which still retained its ductility, being oxidized. Were we to suppose 14·3 grains of that portion to be oxidized (and some of it certainly was, as it had all lost its lustre) it would reconcile this experiment with the preceding.

One experiment described.

But if the 236·63 grains of iron had combined with 69 grains of oxigen, they ought to have weighed 305·63 grains. But the actual weight was found to be 330·68 grains, or 25 grains heavier than they ought to have been from theory. This increase of weight, which was constant in all my trials, cannot be accounted for on the present universally received chemical theory; unless we suppose, that a little water, as well as oxigen, has actually combined with the iron—a supposition which was strenuously maintained by Dr. Priestley. I attempted to ascertain exactly how much of the water had disappeared in a similar experiment, but the apparatus used was so bulky, that I could not weigh it with sufficient precision, to determine so delicate a point.

Increase of weight above that of the oxigen expended.

Was this owing to water combined with the iron?

5. From the whole of these experiments it seems to follow, that black oxide of iron is composed of 100 parts of metal and about 27·5 of oxigen.

III. When iron is dissolved in diluted sulphuric acid, if it be precipitated by an alkali, a white powder falls, which

Thenard's white oxide of iron,

Thenard

Thenard considers as a peculiar oxide. According to him there are three oxides of iron, the *white*, the *green*, and the *red* *. I prepared a quantity of this supposed *white* oxide with all the requisite precautions, but on attempting to dry it, the colour soon changed. It became first green, then black, and last of all red. 100 grains of iron treated in this way were converted into 158·4 grains of a red powder, which lost no weight in a red heat. This red powder contained a good deal of sulphuric acid; for, when dissolved in muriatic acid, muriate of barytes threw down a copious white precipitate. Hence it is obvious, that the supposed white oxide is a subsulphate of iron. In my experiment the quantity of sulphuric acid present was about 13·4 grains. If sulphate of iron reduced to powder be digested in alcohol, it is converted into a similar white subsulphate.

a subsulphate.

IV. In some of my experiments on the ores of iron, the result which I obtained did not correspond with the notion which I entertained of the composition of black oxide of iron: the oxide examined contained less oxigen. Hence I concluded, that there was an oxide of iron in nature containing less oxigen than black oxide. But it is obvious, that what I at that time considered as a new oxide is in reality black oxide, and that my black oxide was in reality a mixture of the black and the red. I allude to my analysis of iserine and of iron sand; published some time ago in the 6th volume of the Transactions of the Royal Society of Edinburgh†.

Supposed native protoxide.

V. I know not whether I ought to notice a remark, with which Mr. Hassenfratz concludes the paper quoted in the beginning of this dissertation. "Dr. Thomson," says he, "or his translator, employs in the passage above quoted two new words; 1. *protoxide* to signify the oxide with a minimum of oxigen; 2. *peroxide* for the oxide with a maximum of oxigen." Fourcroy and Hauy, he tells us, had already used the word *oxidule* to denote the black oxide. He then proceeds to explain the etymology of the two terms which I employ. The Greek numeral πρῶτος prefixed to

Strictures on the author's nomenclature by Hassenfratz

* Journal, vol. xiv, p. 224.

† See Journal, vol. xxviii, p. 19.

oxide,

oxide, he says, constitutes the first; and the Greek preposition ὑπερ prefixed constitutes the second. Had Mr. Hassenfratz taken the trouble to consult my work, volume 1, p. 140, (3d. edition) he would have seen, that *peroxide* was formed by joining the Latin preposition *per* to the word *oxide;* and that, according to a very common use of that preposition in composition, the word *peroxide* means a metal thoroughly oxidized, or saturated with oxigen. He then proposes to substitute for the words *protoxide* and *peroxide* the words *microsoxide* and *megaloxide*, which he says are much more precise. I believe it to be unnecessary to make any observations on this proposed substitution. In what respect these words are more precise than mine, or indeed so precise, I am at a loss to conceive. They signify literally *little oxide* and *great oxide*, phrases which lead us rather to attend to the bulk of the substances, than to the proportion of oxigen which they contain. But even supposing them equally or even more precise, still they could not be substituted for mine; because we require a method of naming all the oxides of a metal, even when they exceed two. My method supplies such a nomenclature; but Mr. Hassenfratz's method, even if we were to introduce also his words *oxidule* and *oxidisque*, supplies no such nomenclature. The same insurmountable objection applies to the *oxidule* of Fourcroy and Hauy. Besides, Mr. Hassenfratz forgets, that the term *oxidule*, though it does well enough in French, may not be suited to other languages. For instance it would neither be introduced into English nor German, without doing violence to the genius of both languages.

answered.

VI. The preceding experiments were made about a year ago; indeed immediately after perusing Mr. Hassenfratz's dissertation. I publish them at present, to put the chemical public on their guard respecting the inaccurate statement of the composition of oxides of iron, which I have introduced into my *System of Chemistry*. I inserted the result of them in the appendix to the 4th edition of that work; but thought it requisite likewise to publish the details, that those who are in possession of preceding editions may be aware of the inaccuracy and correct it.

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THE Authors of Original Papers and Communications in the present Volume are J. A. De Luc, Esq. F. R. S.; G. Cumberland, Esq.; Marshall Hall, Esq. F. R. M. S. E.; Mr. Jos. Hume; Justus; C.; Mr. John Dalton; Mrs. Agnes Ibbetson; Robert Lyall, M. R. P. S. E. &c.; Mr. J. Murray, Lecturer on Chemistry, Edinburgh; A. B.; Thomas Knight, Esq; W. Moore, Esq; the Rev. William Gregor; Mr. John Davy; L. O. C.; J. Bostock, M. D.; Thomas Forster, Esq.; F. D.; F. R. Gowar, Esq.

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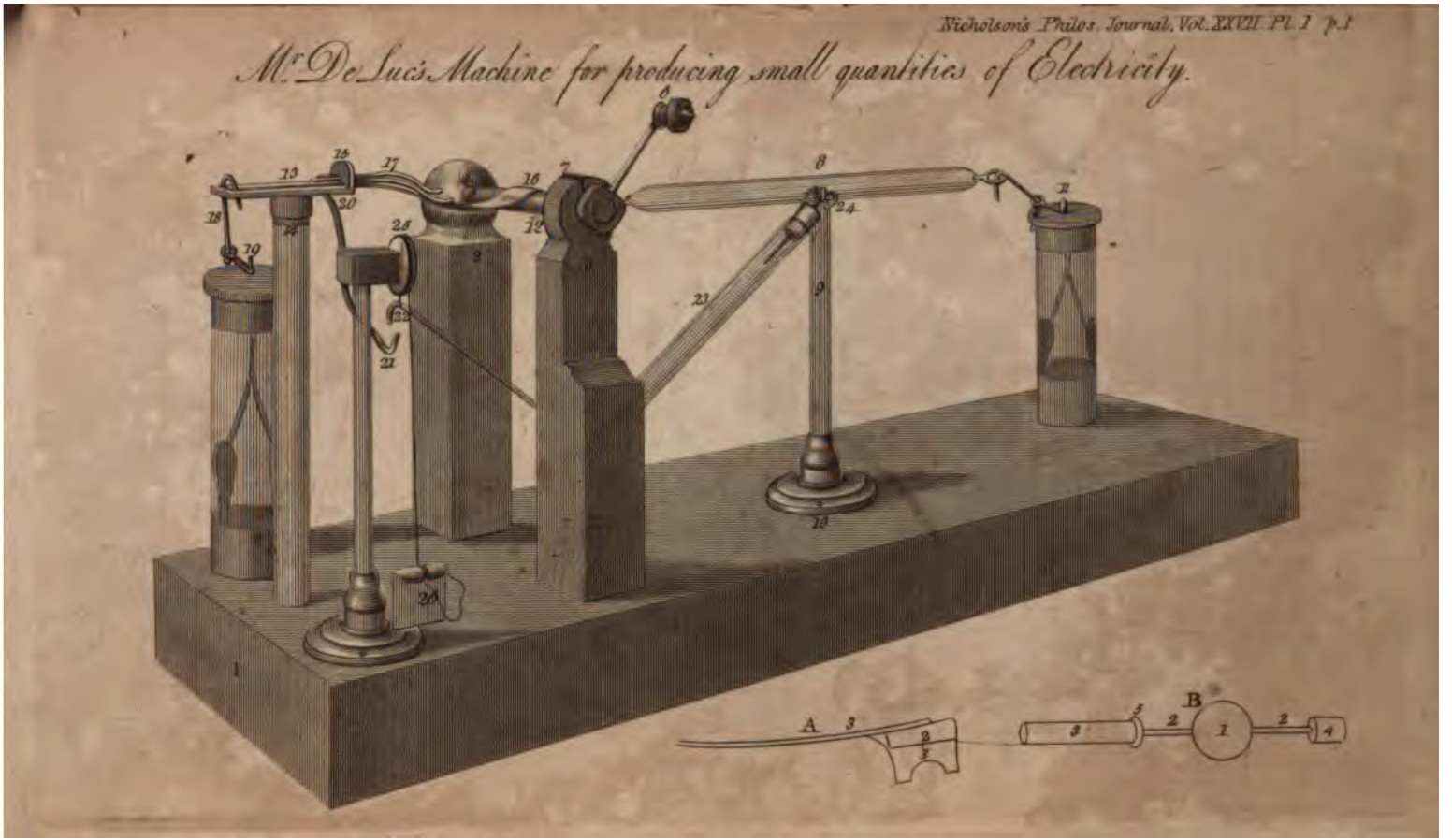
Mr De Luc's Machine for producing small quantities of Electricity.



A

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THE ARTS.

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ARTICLE I.

Experiments concerning the Electric Machine; *showing the Electric Effects of* Friction *between Bodies. By J. A.* De Luc, *Esq. F. R. S.*

THE invention of the *electric machine*, which has been within my time, and of which I have followed the progress, was, with respect to the long known effects of *friction* on *amber* and other substances, what Sig. Volta's *pile* has been to the discovery made by Sig. Galvani of the *motion* produced in the limbs of a recently dead frog by the association of two proper *metals*: I mean, that, by increasing these effects, a particular *fluid* has been found to be the cause of both. However, though this *fluid* has been so long discovered by the *electric machine*, its nature has not yet been agreed upon among all experimental philosophers.

Invention of the electric machine.

History and progress of the invention.

A rapid motion communicated to small globes of different substances, rubbed by the hands, was found to produce certain effects susceptible of being propagated at an unlimited distance through metallic wires, suspended by silk, or supported by glass; and these effects consisted, not only in greater *motions* of bodies than were produced by the *friction* of *amber*, but in strong *luminous* phenomena.

Two kinds of electricity.

The greatest effect of *friction* before observed having been produced by *resinous bodies*, globes were made of them, especially of *sulphur*, for electric machines, at the same time as globes of *glass*, and they both produced the *divergence* of pairs of *balls*; but it was soon observed, that the *divergence* produced by one kind of globe was destroyed by the other kind: whence arose the system of two kinds of *electricity*, which were supposed to *neutralize* each other when *united*, and to be manifested only when separated, producing then their peculiar effects; one of them was called *vitreous* and the other *resinous*.

This new agent,

This new object of study engrossed the attention of all natural philosophers, not only on its own account, but because of the deficiency of known *agents* in a great number of natural phenomena; and this interest having been increased, when the analogy between *lightning* and some of the effects produced by the *electric machine* was discovered, no wonder that many hasty systems were formed for applying this new, but undetermined *agent*, to various phenomena not yet satisfactorily explained, even to the motions of celestial bodies.

Franklin's theory,

The idea of two different *electricities* continued predominant among natural philosophers, till Dr. Franklin explained the difference between the electric phenomena attributed to this double cause by only *more* or *less* (*plus* or *minus*) of the same fluid: but as this ingenious experimental philosopher assigned the intermediate state between *plus* and *minus* to a certain *quantity* of *electric fluid* belonging to bodies, his theory, though adopted by the majority of electricians, remained subject to insuperable objections under this form; which objections nobody more forcibly urged than Dr. Peart, to whom no solid answer has been made, nor could be made, as long as the theory remained on the same foundation. Thus many experimental philosophers have retained the idea of two distinct *fluids*, as producing these two effects which destroy each other: but different hypotheses have been framed on the nature of these fluids, which have been even transformed into certain properties belonging to bodies. However the duration of this variety of systems proceeds only from a want of attention to the complete

exposed to strong objections,

till removed by Volta.

complete solution of all the difficulties, which occurred in the system of *plus* and *minus*, given by Sig. Volta, in fixing the real point to which these *positive* and *negative* quantities are to be referred, namely, the *actual*, but different at different times, *electric state* of the *ambient air*.

Only one electric fluid.

According therefore to this system, all the *electric* phenomena are produced by one *fluid*, the modifications of which in terrestrial phenomena deserve the most attentive and assiduous study of all experimental philosophers: but no agreement among them, either on the importance of this study, or on the mode of its pursuit, can be expected, as long as they shall be so far divided in their opinions on the nature of this agent, as to leave a doubt whether it consists of one and the same, or two distinct *fluids*, or even of some properties of bodies.

Experiments on the effects of friction.

Friction between bodies being the only method, hitherto known, by which can be produced at will the two kinds of *electric signs*, I thought that the most direct and sure method of coming to a better knowledge of their meaning was to study them at the origin of the two distinct phenomena; and I undertook a series of experiments on the effects of *friction* between various kinds of bodies; *insulating* those which are more or less *conductors*, and applying *electroscopes* to both the bodies which exercised *friction* on each other; in order to observe the modifications thus produced, not only on the *nonconducting*, but on the *conducting* bodies. I made these experiments with large apparatusses, by which the reciprocal effects of *friction* between various bodies were manifested by the motions of pairs of balls; and from them I derived, without exception, the theory which I shall first explain, and then prove by an account of some of the principal experiments.

Theory. Conductors and nonconductors.

I. The *electric fluid* resides on all terrestrial bodies, every particle of *air* included; being retained upon them by a mutual attraction, which however differs in degree, according to the bodies: some attract the *electric fluid* only when it comes into contact with them, but then it adheres strongly to the parts which receive it, moving very slowly along the surface of these bodies, which therefore are *nonconductors*: others receive it at more or less distance, and it is propa-

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gated more or less rapidly along their surface. *Glass*, though absolutely *impenetrable* to the *electric matter* (as I have explained in the preceding paper) permits the *electric fluid* (i. e. *electric matter* united with *vector*) to move with a sensible progress along its surface.

Effects of friction.

2. Friction excited between two bodies has no other effect than that of disturbing the natural *equilibrium* of the *electric fluid*, which tends always to be produced among all bodies according to its *actual* (but local, in a certain extent) quantity on them and in the *ambient air*. If both the bodies, which exercise *friction* on each other, are good *conductors*, this disturbance, the equilibrium being constantly restored, is not perceived; but if one has more disposition than the other to attract the *electric fluid* thus agitated, with the faculty of transmitting it to its remote parts; when they are separated, either suddenly, or in general before the *equilibrium* of the fluid can be restored between them, one is found *positive*, as having acquired a proportional quantity of *electric fluid* greater than the *ambient air*, and the other *negative*, as having lost that quantity; both being supposed to have previously possessed the same electric state as the *ambient air*.

3. The general effect therefore of *friction* between two bodies is, that one never becomes *positive*, without the other being made *negative* (or vice versa). This evident proof, that all the *electric phenomena* which we are hitherto able to produce at will, namely by *friction*, proceed from the disturbance of the *equilibrium* of only *one fluid*, will be afforded by the experiments, which I shall here relate after another remark.

Negative and positive not properties of different bodies, but different states of the same body.

Mr. Cavallo, in his *Complete Treatise on Electricity*, (3d edit. vol. i, p. 21.) has given a table containing the results of his experiments of this kind, wherein is found, that certain bodies become either *positive* or *negative*, according to those by which they are *rubbed*. This circumstance had already shown, that *negative* and *positive* were not properties belonging to certain bodies, but only different states produced on the *same* body by different circumstances, and in particular by the difference of those which exercise friction on it: however, there remained to be known what effect, in the latter

latter case, was produced on each of the bodies which exercised that *friction*. This had been one of my objects in the above mentioned experiments; for which purpose I kept *insulated* both bodies exercising *friction* on each other, applying to them *electroscopes*. It is by experiments thus performed, that I found the general law above expressed; for when any of the bodies, which may be rendered either *positive* or *negative* by *friction*, was brought to one of these states, the body which had exercised it was constantly found in the opposite state: if the former became *positive*, it had *taken* some *electric fluid* from, and if *negative*, it had *yielded* some *fluid* to, the body which had exercised *friction* upon it. Such are the effects, which will be seen in the following experiments.

Apparatus contrived to prove the similarity between the electric machine and pile

The small apparatus, which I am going to describe, was not at first intended for this purpose; my view in its construction having been to prove the kind of analogy which I have established in my former paper, between the *electric machine* and the *electric column*, and even the *pile*, with respect to its *electric* property; the only difference in this respect between the electric machine and the latter instruments being, that, as in the former the effect is produced by *friction*, it requires a mechanical process for its continuance; while in the *column* and in the *pile* it is produced by only the association of two proper *metals*. The surest mode of following the course of the *electric fluid*, when its equilibrium is disturbed, is by the motions of *electroscopes*; but the immediate comparison which I had in view could not be made with any of the usual *electric machines*; as the *gold-leaf* being the only *electroscope*, that can be effected by a *pile* or *column* of a moderate size, it would be torn to pieces by the smallest *electric* machine hitherto used. This made me think of the construction of so small an *electric machine*, that the *gold-leaf* electroscope should not be more affected by it than by my largest *column*; but after the first experiments I was induced to construct this small apparatus so as to be applied to experiments concerning the different effects of *friction* between different bodies.

This apparatus described.

The dimensions of this instrument, as represented in the figure Plate I, are, in the parts supposed the nearest to the

Apparatus described.

the spectator, half those of the original; the other parts diminishing as required by perspective. A wooden base 1, 1, on which the different parts of the apparatus are fixed, must, during the experiments, be fastened to a table, by proper vices, in order to make it steady while the instrument is worked. On this base are solidly fixed two wooden pillars, 2 and 3, the former of which is bored a little way in, at the top 4, in a horizontal direction, for receiving one end of different *spindles*, as 5, the other end of which is lodged in a groove at the top of the pillar 3, and projects outwards to receive a winch 6, by which all the *spindles* are made to revolve: they are prevented from moving upwards in the groove by a piece of wood 7, placed when they have been laid in, and outwards, by a ridge on the *spindle*, bearing against the pillar 3 (as 5, fig. B.). The *prime-conductor* 8, is made of a *brass* tube stopped at each end; it is supported by a *glass* pillar 9, covered with insulating varnish, and fixed, on the base 1, 1, by its own wooden base 10. One end of this *conductor* communicates with the *spindles* of whatever kind, by means of a very thin slip of whale-bone covered with gilt paper, fixed to a brass piece, which screws into the end of this *conductor*: this screw has some length, in order to move it backwards or forwards, so that the extremity 12 of the whale-bone slip may attain the proper point of the *spindle*: the slip, which is elastic, presses against the *spindle* by a natural bent upwards. This part of the apparatus is the most difficult to adjust; the proper bearing of the extremity 12 of the slip, on the different *spindles*, must be found by trials; in general, it must bear, beneath the *spindles*, at a small distance from the extremity 16 of the various *rubbers*. The other end of the *prime-conductor* communicates with the *gold-leaf* electroscope 11.

There are different kinds of *rubbers*, but they are all fixed in the same manner to the apparatus: they consist, either wholly, or in great part, of a very elastic *brass* lamina, and differ only by that end of them, 16, which presses on the *spindle*: the other end enters laterally, and is fixed by a wedge, into a notch horizontally cut in the part 15 of a brass piece 13, held on the top of a glass pillar, 14, covered

covered with an insulating varnish, and solidly fixed into the wooden base, 1, 1. The different kinds of *rubbers* must press with different degrees of force, according to their nature, and yield always easily to the inequalities of the *spindles*. For these purposes, a part of the pressure is exercised by one or two additional brass springs, as at 17, pressing on the main spring 16. The *brass* piece 13, by the wire 18, serves as *conductor* from the different *rubbers* to the *gold-leaf* electroscope 19. Another wire, fixed to the same piece at the point 20, projects from it, and is terminated by a hook 21: this hook is to receive, occasionally, the hook 22, of a wire belonging to a *glass tube* filled with water 23, the other wire of which is hooked to the *prime conductor* at the point 24. The hook 22 is commonly held up by a silk thread, passing over an insulated pulley 25, and stopped at the bottom by a bead fixed to the thread, in a notch of the brass piece 26; the bead is disengaged when the hook 22 is to come down into the hook 21.

Such is the general description of the apparatus, and I come to the experiments; beginning this account by those which I had principally in view in its construction, namely, to have an *electric machine* with a power not exceeding that of my *electric column* of 600 groups, for the purpose of comparing their correspondent effects. I made many trials before I could bring an *electric machine* down to so small a power; the following was that which nearly succeeded, though still too strong.

Difficult to reduce its power to that of the column.

Exp. 1. The *spindle*, 5, is here a solid *glass* cylinder very little above $\frac{1}{4}$ of an inch diameter; it is covered with *sealing wax* at both ends, for the purpose of insulating a naked part, about $\frac{1}{4}$ of an inch in length, in the middle of which the *friction* is produced. The *rubber* is a simple *brass* spring, so bent at its extremity, 16, as to fit the upper part of the *glass* cylinder.

Exp. 1.

This apparatus in fact is an *electric machine*, with a *glass cylinder* of only $\frac{1}{4}$ of an inch diameter; however, small as it is, it must be moved very slowly, or the *gold leaves* would have greater motions in striking the sides, than they have at the extremities of my *column* of 600 groups, even in the time

Shows the small quantity of the electric fluid acting in the pile.

time of its greatest action. This experiment therefore directly shows how small is the quantity of *electric fluid*, which, modified in the *pile*, produces such great *chemical* and *physiological* effects; and the following will prove, that the *motion* of the *electric fluid* is the same in both instruments, though produced by different causes.

Exp. 2.

Exp. 2. While the machine is in motion, if I loosen the silk thread which holds up the hook, 22, of the wire belonging to the *glass tube* with water, 23, in order that it may fall into the hook, 21, and that the communication may be thus established between the *rubber* and the *prime conductor* through the *water* of the *glass tube*; at the instant that the *hooks* come into contact with each other, the very *small* quantity of *electric fluid* there in motion pervading the *water*, the *gold leaves* fall in both electroscopes, and never rise though the machine be kept in motion. This is the same effect as is produced on the *column* in *exp.* 29 of the former paper; for the divergence ceases also in both its electroscopes, when a similar *tube* connects its extremities: in both cases, the *electric fluid*, which tends to accumulate on one side, passes instantly to the other, and thus keeps up the equilibrium. That such is the effect of the *circulation* of only *one* and the same *fluid* set in motion, by *friction* in the *electric machine*, and by the association of the two *metals* in the *column*, will be proved by the effects of *friction*, an instance of which is already seen in the former case.

Proofs that only one fluid circulates.

In the above experiment, *friction* is exercised between two bodies, one of which ranks among the best *conductors*, namely *brass*; however the effect is observed upon it, because it is *insulated*: the other is the *glass* cylinder, which, passing under the *brass* rubber, certainly takes *electric fluid* from it, since, transmitting some to the *prime conductor*, and by this to the electroscope 11, the latter diverges *positively*; and since at the same time the *brass* takes off some *fluid* from the electroscope 19, which thus diverges *negatively*.

Effect of the amalgam on common rubbers.

This experiment shows moreover what is the effect of the *metallic amalgam* laid on the *rubbers* of the usual *electric machines*. The base of those *rubbers* must be a cushion,

in

in order to yield to the inequalities of the *cylinders* or *plates* while it presses upon them; but the materials of the cushions are not sufficiently *conducting* to permit the loss, which its rubbed part undergoes, of the *fluid* carried off by the *glass*, to be soon repaired by the *ground*; whereas this is effected by the *metallic amalgam*, from which the *glass* takes readily some *electric fluid*, as, in the above experiment, it takes it from the *brass rubber*. Metallic *plates* would produce the same effect on the *cylinders* and *plates* of *electric machines*, if the former were sufficiently pliable.

General effect of friction

The general effect of *friction* between two bodies, as I have said above, is, that if one becomes *positive* or *negative*, the other is found in a contrary state. This holds not only between bodies of different natures, but even between the same kinds of bodies, if one be made to pass in length over one part only of the other. This effect cannot be observed with perfect *conductors*, as on them the *equilibrium* of the *electric fluid* is instantly restored; but there is a known experiment made with two pieces of the same *silk riband*, in which, by making one piece to pass rapidly in length on one part only of the other, the former becomes *positive*, by carrying off some *electric fluid* from the latter, which thus is rendered *negative*, by losing that fluid. I have tried the same experiment with *glass*, in the following manner.

exemplified with two pieces of riband, and

Exp. 3. At the end of a brass spring I fixed a flat piece of the same *glass* as the small cylinder, and used it as a *rubber*, in the same manner as in the former experiments. Both pieces of *glass* being therefore *insulated*, there was no sensible effect on either of the electroscopes; as the immediate effect and consequence of *friction* between these small bodies, being confined within the bodies themselves, were too small to be perceived. I then held my finger on the brass spring near the flat piece of *glass*, in order to restore to the latter, from the ground, the *fluid* carried off by the *glass* cylinder; and having turned rapidly the winch for a little time, the electroscope 11 diverged *positively*. This is the same effect as produced on the *riband* passing rapidly over a part only of another similar: the *glass*, which turned, carried off some *electric fluid* from the other *glass*, which was rubbed only on one point; and as the latter now repaired

Exp. 3, with glass.

paired its losses from my finger, the cylinder, which received more *fluid*, communicating thus more of it to the prime conductor, that quantity became sensible to the electroscope 11. The *glass* rubber, undergoing these *losses*, was at every instant rendered *negative*, but not in a perceptible manner, because its losses were also instantly repaired from my finger, which, being in the way, prevented the electroscope 11 from showing this state.

Exp. 4. Friction between glass and sealing wax.

Exp. 4. I covered with *sealing wax* the under part of the extremity 16 of a brass rubber, similar to that of Exp. 1, and made the *wax* rub the *glass* cylinder; this produced a little divergence in the electroscope 11, which was *positive*, but none in the electroscope 19. This at first appears contrary to the above law, namely, that one of the bodies, which *rub* each other, can acquire some *electric fluid* only by taking it from the other, which must become *negative*. But afterward I considered, that the *sealing wax*, here a mere lamina, covering the *brass* spring, being impermeable to the *electric fluid*, the quantity carried off by the *glass* cylinder from its free surface could not be repaired *through* it by the *brass*, which therefore lost no sensible quantity of the *fluid*. In consequence of this idea, I changed the *rubber* in the manner represented by the separate *fig.* A. A piece of *sealing wax* about $\frac{1}{4}$ of an inch thick, cut underneath so as to fit the *glass* cylinder, is held in its upper part by a doubled piece of thin *brass* (one side of which is seen in 2); and on its top is soldered a *brass* spring 3, the opposite end of which is placed, as usual, in the *brass* piece 13, communicating with electr. 19. By this construction, while the *sealing wax* loses some *electric fluid* to the *glass* cylinder which revolves against it, this loss is repaired by the *fluid* proceeding from the *brass* (as from 2, fig. A), moving *along* the side of the piece of *sealing wax*, which thus is enabled to yield more *fluid* to the *glass* cylinder. Thus the electroscope 11 has a much greater *positive* divergence than before, and the electroscope 19 has now a great *negative* divergence, because the *brass* spring, communicating with it, loses some *electric fluid*, by yielding it to the *sealing wax*.

Exp. 5.

Exp. 5. In a piece of *brass* similar to the above, *fig.* A, instead of *sealing wax*, I placed a piece of *Caoutchouc*, or *indian rubber* of the same shape as the wax. This body produced here a singular phenomenon (observed in other cases) which characterizes the manner in which *friction* acts in these phenomena; its effects changing, according to circumstances foreign to the intrinsic nature of bodies. This experiment is difficult, on account of the tendency of the *indian rubber* to stick to the *glass cylinder*, by which the former is torn or disfigured if the pressure is too great, or the motion too rapid: it is therefore necessary to find the proper degree of both. Now, according to these (or other unperceived circumstances) having repeated this experiment on different days, sometimes the *glass* took off some *electric fluid* from the *indian rubber*, and made the electroscope 11 diverge *positively*; at other times it was the reverse; and in both cases the *indian rubber* was in the state opposite to that of the *glass cylinder*, as shown by the electroscope 19. Exp. 5. Glass and india rubber.

Exp. 6. It has been seen in *exp.* 1, that a *brass* rubber, applied to the *glass* cylinder, becomes *negative*, the latter becoming *positive*. I covered a similar *glass* cylinder with a thick coating of *sealing wax*, and applied to it a *brass* rubber of the same *breadth* as the former, fitted to the *sealing wax* cylinder: it produced no sensible effect. I suspected, that this *rubber* was too *wide*: because thus embracing, in a great length, half the circumference of the *sealing wax* cylinder, which constantly revolved against it, the *sealing wax* lost and regained alternately *electric fluid* on too great a surface; for the point 12, with which communicates the *prime conductor*, to preserve any sensible change. With this idea, I reduced the *breadth* of the *brass* rubber, in the part bearing on the *sealing wax*, to $\frac{1}{4}$ of an inch; placing the point 12 of the *prime conductor*, immediately under and very near the extremity of the *rubber*. Then the electroscope 11, communicating with the rubbed *sealing wax*, diverged *negatively*, and the electroscope 19, communicating with the *brass* rubber, diverged *positively*. Exp. 6. Brass and sealing wax.

This experiment shows, in the first place, the errour of a distinction between bodies as *electrics* and *anelectrics* Distinction of electrics per se improper.

{electric

(electric *per se*, and electric *by communication*, as they have been also called); considering *friction* as the test between these two classes of bodies, and placing *metals* foremost among the *anelectrics*. But it is seen in the above experiments, that there is no such distinction, since *brass* undergoes, by *friction*, changes in its *electric* state, as well as *resinous* bodies and *glass*: only these changes, in conducting bodies, cannot be manifested without *insulation*; and the want of this condition in the experiments of former electricians was the cause of the errour.

Neither are there two kinds of electricity.

The same experiments show also, that *brass* (as a *metal*) becomes either *positive* or *negative*, according to the body with which it undergoes *friction*. In the above experiment the *brass* rubber was made *positive*, because it took off some *electric fluid* from *sealing-wax*; but in *exper.* 1 it was made *negative*, because it lost some of the *fluid* to *glass*. But this difference in the *electric* state of the same body, by the difference of those with which it undergoes *friction*, extends farther; and the following experiment will show the errour of the distinction between two electricities, in which one is called *resinous* and the other *vitreous*; and in general between two *fluids*, or two *electric* properties in different bodies.

Exper. 7. Sealing wax and india rubber.

Exper. 7. It has been seen in the last experiment, that the *sealing-wax* cylinder was made *negative*, by being *rubbed* with *brass*. To the same cylinder I applied the *indian rubber*; and with the smallest pressure (to prevent adhesion), and a very slow motion of the winch, the gold leaves struck so rapidly in both electroscopes, that, in order to try the kinds of *divergences* produced, I was obliged to stop at one part only of a revolution: then I found constantly, that the *sealing-wax*, which in the above experiment was rendered *negative* by *brass*, now became *positive*; and that the *indian rubber*, which in *exper.* 5 became sometimes *positive* with the *glass* cylinder, was here constantly *negative*. Therefore *sealing-wax* itself either *loses* or *gains* some *electric fluid*, according to the body which exercises *friction* upon it, and the *rubbing* bodies always experience the contrary effect.

I have

I have made various experiments of the same kind, with various bodies, all affording the same general result; but I shall only relate one more, giving another example of a body which becomes either *positive* or *negative* according to the other body with which it undergoes *friction*. This is one of the beads, of the size and colour of cherries, worn by indian women, which, as Dr. Lind informs me, consist of an inspissated vegetable oil: they are not soluble by water, and are very elastic.

Farther proof of a body becoming positive or negative.

Exper. 8. I mounted one of these *beads* on a *spindle*, as in fig. B. The *bead*, 1, is traversed by a small *glass* axis, 2, 2, covered with *sealing-wax*, to the ends of which are adapted pieces of wood, 3 and 4, on the first of which is seen, at 5, the *ridge* before mentioned, which prevents the *spindles* from coming out when placed between the pillars of the apparatus.

Exp. 8.

1. With a narrow *brass* rubber, the extremity of which is fitted to the form of the *bead*, the slowest motion of the winch renders the bead *negative*, and the *brass* rubber positive.

2. With a *sealing-wax* rubber like that fig. A, but fitted to the form of the *bead*, with very little pressure and motion, the *bead* becomes *positive*, and the *sealing-wax* negative.

Such are the constant phenomena observed at the fountain head of all the *electric effects*, which it is in our power to produce; and they depend on *friction*, respecting which various systems have been fabricated. Now from the whole tenour of these experiments it may be laid down as a fundamental truth in terrestrial physics, that *friction* has no other influence in *electric* phenomena, than that of disturbing the *equilibrium* of the *electric fluid*, in such a manner, that one body, by *acquiring* a certain quantity of it above what it had before, is rendered *positive*, and that the other is found *negative*, as having lost that quantity. It is only when its *equilibrium* is disturbed, that the *electric fluid* is manifested to us: the *electroscope* is our first test of this disturbance; but if the bodies, either *conductors* themselves, or associated with *conductors*, are of a sufficient size, and the electric difference between them has arrived to a certain

Friction only disturbs the equilibrium of the electric fluid;

and it is then alone manifest to us.

a certain degree, it is manifested by a *spark* darting from one to the other, and the *equilibrium* is thus restored.

I cannot but consider the fixation of this point, after many controversies, as being no less conducive to the advance of our knowledge in the maze of *terrestrial causes*, than the determinations concerning *air* and the *cause of heat*; and no experiments and observations appear to me to deserve more the joint endeavours of experimental philosophers, than those concerning the modifications of the *electric fluid*, in the *atmosphere* and in the *ground*, of which, in my former paper, I have shown the great influence in *terrestrial phenomena*.

Ashfield, near *Honiton*,
12th October, 1810.

II.

Comparative Examination of the Mucous Acid formed by the Action of Nitric Acid on Gums and on Sugar of Milk.

By A. LAUGIER*.

Lime in the immediate principles of vegetables.

MR. VAUQUELIN, by his experiments on gums arabic and tragacanth, has ascertained the existence of a considerable portion of lime in the immediate principles of vegetables. The reading of his paper suggested to me the following reflexions.

What becomes of this in converting gums into mucous acid?

1st, What becomes of the lime contained in these gums, when they are treated with nitric acid, in order to obtain the mucous acid?

2dly, Does it not combine with the oxalic acid, which is formed almost at the same time as the mucous?

3dly, As the oxalate of lime is more insoluble in water than the mucous acid, does it not fall down with this acid when the residuum of the process is washed, and occasion a sensible alteration of its properties?

4thly, How may we ascertain the presence of oxalate of lime in the mucous acid obtained from gums, and separate this calcareous salt from the acid, the purity of which it affects?

* Abridged from the Annal. de Chim. vol. LXXII, p. 81.

T

To solve these questions I undertook the following experiments. Experiments to find this.

I digested 60 gram. [926·4 grs.] of gum tragacanth in eight times their weight of pure nitric acid at 36° [sp. grav. 1·333]; heated the mixture, till it was reduced to the consistence of honey; and then added a sufficient quantity of water. Gum tragacanth converted into mucous acid.

This would not dissolve a white, pulverulent substance, which, collected on a filter and dried in the open air, weighed 9·5 gram. [146·7 grs]. This was mucous acid. The liquid containing the soluble portion of the mixture was of a yellow colour. I evaporated it, till the moment I perceived a slight pellicle form on its surface, when I removed it from the fire. As it cooled, it deposited a great quantity of crystals, some in laminæ, others in needles easily perceived to be oxalic acid. To separate this acid from the mucous, I poured on the mixture alcohol at 40° [sp. grav. 0·817], which dissolved the oxalic acid, without acting on the mucous. This, collected on a filter, weighed 2·1 gram. [32·4 grs]. The alcoholic solution, being gently evaporated, yielded a coloured mass, which I dissolved in water to obtain whiter and purer crystals. Oxalic acid also formed,

The mother waters of this second portion of mucous acid and of oxalic acid contained a mixture of oxalic and malic acids, which I separated by means I shall not mention, as they are foreign to my subject. and malic.

The first portion of mucous acid I obtained was very white. When dried it had the clotted appearance of starch. This I employed in my experiments, rejecting the second portion, which did not appear to me so pure. The mucous acid

To find whether there were any oxalate of lime in this mucous acid, I mixed one part of the nitric acid above-mentioned with ten of distilled water, and poured it on the mucous acid. I then exposed the whole to a heat of 40° or 50° [104° or 122° F.] for 48 hours, shaking it occasionally, to promote the action of the menstruum. The supernatant liquor being poured off, ammonia immediately threw down from it a white earthy salt in silky filaments, which had all the physical properties of oxalate of lime. contained oxalate of lime.

A second

A second portion of weak nitric acid, left to stand on the residuum the same length of time, yielded a fresh quantity of oxalate of lime on the addition of ammonia.

It required eight portions of diluted nitric acid in succession, completely to free this mucous acid from oxalate of lime. The quantity was each time less however, and the ninth portion showed scarcely any perceptible indication of it.

The eight precipitates together amounted to 2·5 gram. [38·6 grs.]

This proved by examination of the salt.

It was of importance to ascertain, whether this foreign matter, which had the appearance of oxalate of lime, were in reality this salt. I therefore boiled it with a solution of saturated carbonate of potash, and when the mutual decomposition of the two salts appeared to me complete, I collected on a filter the portion that had precipitated. This precipitate, not so white as the former calcareous salt, and in coarser powder, dissolved with great effervescence in nitric acid. Its solution, which was acrid to the taste, was not precipitated by ammonia, but very copiously by oxalate of ammonia.

The supernatant liquid, which contained an excess of carbonate of potash, was supersaturated by acetic acid, evaporated to dryness, and the residuum treated with alcohol, in order to separate the acetate from the oxalate of potash, which is not soluble in this menstruum. The mixture, being heated a few moments, was filtered, when the acetate of potash passed through with the alcohol, and the oxalate remained behind.

The matter insoluble in alcohol was dissolved in distilled water; and a drop of this solution, added to half a spoonful of lime water, threw down a pulverulent precipitate, evidently of oxalate of lime. The same solution yielded by evaporation crystals of oxalate of potash.

The experiments I have described leave no doubt of the nature of the calcareous salt, that affects the purity of the mucous acid obtained from gum tragacanth.

Other gums similar.

The same experiments repeated on gum arabic, and on that called in the shops gum of Bassorah, which does not dissolve in water, afforded me nearly the same results.

I observed,

I observed, that, in proportion as the mucous acid was deprived of the oxalate of lime, that rendered it impure, it acquired a more flocculent appearance. Alteration in the appearance of the acid.

To ascertain, whether the mucous acid obtained from the sugar of milk, in which it was first discovered by Scheele, were likewise contaminated with oxalate of lime, I boiled a similar quantity of sugar of milk with eight parts of nitric acid of the same strength as before. The first portions of mucous acid formed I separated by decantation, and added to the residuum a fresh quantity of nitric acid. A second portion of mucous acid was deposited, which, added to the first, made up the weight of 12 gram. [185·3 grs.], or a fifth of that of the sugar of milk employed. Mucous acid prepared from sugar of milk, by way of comparison.

I remarked, that this mucous acid, after washing, when diffused in water had as flocculent an appearance as that from the gum, when it had been deprived of its oxalate of lime by dilute acid. This led me to presume, that the acid was much purer than that from gum; and my conjecture was soon confirmed by nitric acid having no action on it. This acid could not take up from it the smallest quantity of oxalate of lime in a long continued digestion, for ammonia did not produce the slightest cloud in the supernatant liquid. This apparently pure.

What besides left no doubt of the perfect purity of the mucous acid from sugar of milk was its easy and complete solution in boiling water. This even proves it to be more pure than the mucous acid from gum, after it has been deprived of oxalate of lime by the means mentioned above: for the latter, when boiled with distilled water, leaves an insoluble flocculent matter, amounting to 0·06 of its weight; which dries into a gray, horny, semitransparent substance, much resembling in appearance the mucous matter, that connects together the particles of animal concretions; though, when thrown on burning coals, it does not emit the ammoniacal and fetid smell of animal compounds, and yields by calcination carbonate of lime. The very small quantity I obtained did not allow me to make such experiments, as would furnish a more accurate knowledge of the nature of this substance. The acid from gum not perfectly pure when freed from oxalate of lime, as it contained another substance.

From the facts that have been mentioned, we may infer, General conclusions.

1st, There is a striking difference between the mucous acid obtained by the action of the nitric acid from gums, and from the sugar of milk.

2dly, This difference consists in the former being always contaminated with a mixture of oxalate of lime, proportional to the lime contained in the gum; while the mucous acid from sugar of milk does not exhibit the least trace of this calcareous salt, and seems to be perfectly pure.

3dly, The mucous acid from gum may be brought to a similar state of purity by a very simple process, which consists, 1st, in removing all the oxalate of lime by repeated digestions in very dilute nitric acid; and, 2dly, in boiling it in water, which dissolves it, without dissolving the flocculent matter, that the nitric acid did not take up.

4thly, The mucous acid from gum, when thus freed from substances foreign to its nature, is exactly similar to that from sugar of milk, possesses all the properties that characterise this acid, and may be employed with equal advantage in the most delicate experiments, or such as require this acid to be of the utmost purity.

Mucous acid from gum contains mucite of lime, when prepared with diluted nitric acid.

Since I wrote the above I have ascertained, that under certain circumstances the mucous acid obtained from gum is mixed with mucite of lime, instead of the oxalate I have mentioned. This happens, when the nitric acid employed is not in a concentrated state, but diluted with water, and the process consequently goes on not rapidly, but slowly. The difference of the results is easily understood.

Reason of this.

If a weak acid be employed, the mucous acid is at first produced alone; it precipitates with the lime, with which it forms a salt of little solubility; and it may be separated from the mixture previous to the formation of the oxalic acid, which requires the concentration of the nitric. If on the contrary concentrated nitric acid be used, the formation of the two acids, though taking place in succession, is very near in point of time; and it may readily be supposed, that in this case the oxalic acid, in proportion as it is formed, lays hold of the lime in consequence of its great affinity for this earth.

I shall add one more fact, that has led me to remark a singular property of the mucous acid, which I purpose to examine more fully. When

When a solution of pure mucous acid in boiling water is gently evaporated to dryness, without separating the crystalline precipitate, that forms during the evaporation; we soon find, as soon as the whole of the liquid is wasted, that the crystals grow yellow, then brown, and are converted into a kind of viscid substance, tenacious, undergoing a sort of fusion, and acquiring considerable hardness on cooling. Mucous acid changed by heat.

The mucous acid, that has experienced this change, is much more sour than usual, infinitely more soluble in water, and wholly soluble in alcohol, so that its properties are in part altered. At first I thought, that I had converted the mucous acid either into the malic, or the tartaric; but the experiments I have made to verify this conjecture do not yet appear to me sufficient, to authorise my advancing any opinion respecting the nature of the change, that takes place in the experiment I have described. Its properties in this state.

III.

Chemical Analysis of a Black Sand, from the River Don, in Aberdeenshire; and of a Copper Ore, from Airthrey, in Stirlingshire. By* THOMAS THOMSON, *M. D. Lecturer on Chemistry, Edinburgh* †.

THE specimen, which formed the subject of the first of the following *analyses*, was brought from the banks of the river Don, about seven years ago, by my friend Mr. James Mill, who at that time resided in Aberdeenshire. By him I was informed, that considerable quantities of it are found in different parts of the bed of that river,—that it is called by the inhabitants *iron-sand*,—and that they use it for sanding newly written paper. I tried some experiments in the year 1800, in order to ascertain its nature; but was too little skilled at that time, both in mineralogy and practi- Black sand on the banks of the Don.

* For an account of a black sand, consisting of a ferriferous ore of titanium, found on the shores of Liguria, and traced to the rock from which it proceeded, see our Journal, vol. XXVI, p. 94.

† Trans. of the Royal Soc. of Edinburgh for 1807.

cal chemistry, to manage an analysis of any considerable difficulty.

mixed with the detritus of granite or gneiss,

The black powder is mixed with a good many small, whitish, reddish, and brownish grains, which, when examined by means of a glass, prove to be pieces of quartz, felspar, and mica. From this it would appear, that the sand of the river Don consists chiefly of the detritus of granite or gneiss.

and consisting of two substances,

When a magnet is passed over the sand, some of the black grains adhere to it, and are by this means easily obtained separate. But after all that can be attracted by the magnet is removed, the greater part of the black powder still remains. This residue is indeed attracted by a powerful magnet, but so very feebly, that it is not possible by means of it to separate it from the grains of sand with which it is mixed. Thus we learn, that the black matter consists of two distinct substances; one of which is powerfully attracted by the magnet, the other not. As this second substance was obviously specifically heavier than the grains of sand with which it was mixed, I placed a quantity of the powder on an inclined plane, and by exposing it cautiously, and repeatedly, to a jet of water, I succeeded in washing away most of the grains of sand, and thus obtained it in a state of tolerable purity.

iron-sand and iserine.

The first of these minerals we may call *iron-sand*, and the second *iserine*, as they belong to mineral species, which oryctognosts have distinguished by these names.

The iron-sand described.

The iron-sand is much smaller in quantity than the iserine, and does not exceed one fourth of the mixture at most. Its colour is iron-black. It is in very small angular grains, commonly pretty sharp edged, and sometimes having the shape of imperfect octaedrons. The surface is rough; the lustre is feebly glimmering and metallic; the fracture, from the smallness of the grains, could not be accurately ascertained, but it seemed to be conchoidal. Opake, semihard, brittle, easily reduced to powder. Powder has a grayish black colour; powerfully attracted by the magnet; specific gravity 4·765.

Analysis of it.

1. As acids were not found to act upon this mineral, 100 grains of it were reduced to a fine powder, mixed with twice

twice its weight of carbonate of potash, and exposed for two hours to a red heat, in a porcelain crucible. The mass, being softened in water, was digested in muriatic acid. By repeating this process twice, the whole was dissolved in muriatic acid, except a brownish white matter, which, being dried in the open air, weighed 19¼ grains.

2. The muriatic acid solution, which had a deep yellowish brown colour, was concentrated almost to dryness, and then diluted with water. It assumed a milky appearance; but nothing was precipitated. Being boiled for some time, and then set aside, a curdylike matter fell. It was of a milk-white colour, weighed, when dry, 7 grains, and possessed the properties of oxide of titanium. Oxide of titanium.

3. The residual liquid being supersaturated with ammonia, a dark reddish-brown matter precipitated, which being separated by the filter, dried, drenched in oil, and heated to redness, assumed the appearance of a black matter, strongly attracted by the magnet. It weighed 93·7 grains, and was oxide of iron. Oxide of iron.

4. The 19·5 grains of residual powder, being mixed with four times its weight of carbonate of soda, and exposed for two hours to a red heat, in a platinum crucible, and afterward heated with muriatic acid, was all dissolved, except about a grain of blackish matter, which was set aside.

5. The muriatic solution being concentrated by evaporation, a little white matter was separated. It weighed one fourth of a grain, and possessed the characters of oxide of titanium. More oxide of titanium.

6. When evaporated to dryness, and redissolved in water, a white powder remained, which proved to be silica, and which, after being heated to redness, weighed one grain. Silex.

7. The watery solution being supersatured with potash, and boiled for a few minutes, was thrown upon a filter, to separate a reddish-brown matter, which had been precipitated. The clear liquid, which passed through the filter, was mixed with a solution of sal ammoniac. A soft white matter slowly subsided. It was alumina, and, after being heated to redness, weighed half a grain. Alumine.

More oxide of titanium.

8. The brown-coloured matter which had been precipitated by the potash, when dried upon the steam-bath, weighed 20·2 grains. It dissolved with effervescence in muriatic acid. The solution had the appearance of the yolk of an egg. When boiled for some time, and then diluted with water, it became white, and let fall a curdy precipitate, which weighed, when dry, 4·6 grains, and possessed the properties of oxide of titanium.

9. The residual liquor, being mixed with an excess of ammonia, let fall a brown matter, which, after being dried, drenched in oil, and heated to redness, weighed 6 grains. It was strongly attracted by the magnet, but was of too light a colour to be pure oxide of iron. I therefore dissolved it in muriatic acid, and placed it on the sand-bath, in a porcelain capsule. When very much concentrated by evaporation, small white needles began to make their appearance in it. The addition of hot water made them disappear; but they were again formed, when the liquor became sufficiently concentrated. These crystals, when separated, weighed 1·3 grains, and proved, on examination, to be white oxide of arsenic. During the solution of the 6 grains in muriatic acid, a portion of black matter separated. It weighed 0·2 of a grain, and was totally dissipated before the blowpipe in a white smoke. Hence, it must have been arsenic. These 1·5 gr. are equivalent to rather more than 1 grain of metallic arsenic. Thus it appears, that the 6 grains contained 1 grain of arsenic, which explains the whiteness of their colour. The rest was iron. It can scarcely be doubted, that the proportion of arsenic present was originally greater. Some of it must have been driven off, when the iron oxide was heated with oil.

Arsenic.

Iron.

More oxide of titanium.

10. The insoluble residue (No. 4) was with great difficulty dissolved in sulphuric acid. When the solution was mixed with ammonia, a white powder fell, which weighed 0·8 of a grain. It was accidentally lost, before I examined its properties. But I have no doubt, from its appearance, that it was oxide of titanium.

Results of the analysis.

11. Thus, from the 100 grains of iron-sand, the following constituents have been extracted by analysis:

Black

Black oxide of iron	98·70
White oxide of titanium	12·65
Arsenic	1·00
Silica and alumina	1·50
Total,	113.85

Here there is an excess of nearly 14 grains, owing, without doubt, to the combination of oxigen with the iron and the titanium during the analysis.

Had the iron in the ore been in the metallic state, the excess of weight, instead of 14, could not have been less than 30. For the black oxide is known to be a compound of 100 metal and 37 oxigen. Hence, I think, it follows, that the iron in our ore must have been in the state of an oxide, and that it must have contained less oxigen than black oxide of iron. A good many trials, both on iron-sand, and on some of the other magnetic ores of iron, induce me to conclude, that the iron in most of them is combined with between 17 and 18 *per cent* of oxigen. This compound, hitherto almost overlooked by chemists, I consider as the real protoxide of iron. Thenard has lately demonstrated the existence of an oxide intermediate between the black and the red; so that we are now acquainted with four oxides of this metal. But the protoxide, I presume, does not combine with acids like the others. Analogy leads us to presume the existence of a fifth oxide, between the green and the red.

State of the iron in the ore.

Protoxide of iron.

A fifth oxide presumable.

As to the titanium, it is impossible to know what increase of weight it has sustained, because we are neither acquainted with it in the metallic state, nor know how much oxigen its different oxides contain. It is highly improbable, that, in iron-sand, the titanium is in the metallic state, if it be made out that the iron is in that of an oxide. The experiments of Vauquelin and Hecht, compared with those of Klaproth, have taught us, that there are three oxides of titanium, namely, the blue, the red, and the white. From an experiment of Vauquelin and Hecht, and from some of my own, I am disposed to consider these oxides as composed of the following proportions of metal and oxigen:

State of the titanium in the sand.

1. Blue

Proportions of its oxides.

	METAL*.	OXIGEN.
1. Blue,	100	16
2. Red,	100	33
3. White,	100	49

I find, that, when the white oxide of titanium is reduced to the state of red oxide, it loses one fourth of its weight; and that red oxide, when raised to the state of white oxide, increases exactly one third of its weight. It was the knowledge of these facts, that led me to the preceding numbers. And I think they may be used, till some more direct experiments lead us to precise conclusions.

State of the titanium in the iron-sand.

Red oxide being the only state in which this metal has yet occurred separate, we may conclude, that it combines, in this state, with metallic oxides, and that the titanium in iron-sand, is most probably in this state. But white oxide, diminished by one fourth, gives us the equivalent quantity of red oxide. On this supposition, the titanium present, before the analysis, in the 100 grains of ore, weighed 9·5 grains.

The arsenic from accidental arsenic pyrites.

The appearance of the arsenic surprised me a good deal, as it was altogether unexpected. I am disposed to ascribe it to some particles of arsenic pyrites which might have been accidentally present. This conjecture will appear the more probable, when we reflect, that arsenic pyrites very frequently accompanies iron-sand. Before the microscope, the iron-sand appears to contain some white shining particles, which, probably, are arsenic pyrites.

The small quantity of silica and alumina I ascribe, without hesitation, to grains of quartz and felspar, which had adhered to the iron-sand, and been analysed along with it. Some such grains were actually observed and separated. But others, probably, escaped detection.

12. If these suppositions be admitted as well founded, the iron-sand was composed of

Component parts of the iron-sand.

Protoxide of iron	85·3
Red oxide of titanium	9·5
Arsenic	1·0
Silica and alumina	1·5
Loss	2·7
	100·0

* Probably, from what follows, an errour of the press for OXIDE. C.

The

The loss will not appear excessive, if we consider, that a portion of the arsenic must have been sublimed, before the presence of that metal was suspected.

Upon the whole, I think we may consider the specimen of iron-sand examined, as composed of 9 parts protoxide of iron, and 1 of red oxide of titanium. The presence of titanium in this ore had been already detected by Lampadius, though, as I have not seen his analysis, I cannot say in what proportion.

The colour of the iserine is iron-black, with a shade of brown. It consists of small angular grains, rather larger than those of the iron-sand, but very similar to them in their appearance. Their edges are blunt; they are smoother, and have a stronger glimmering lustre than those of the iron-sand. Lustre semimetallic, inclining to metallic. The fracture could not be distinctly observed, but it seemed to be conchoidal; at least nothing resembling a foliated fracture could be perceived. Opake, semihard, brittle, easily reduced to powder; colour of the powder unaltered; specific gravity 4·491*; scarcely attracted by the magnet. The iserine described.

1. A hundred grains of the powdered ore were mixed with six times their weight of carbonate of soda, and exposed for two hours to a red heat, in a platinum crucible. The mass obtained, being softened with water, dissolved completely in muriatic acid. When the solution was concentrated, it assumed the appearance of the yolk of an egg. It was boiled, diluted with water, and set aside for some time. A white matter gradually deposited, which, when dried on the steam-bath, weighed 53 grains, and possessed the properties of oxide of titanium. Analysis of it. Oxide of titanium.

2. The liquid, thus freed from titanium, was evaporated to dryness, and the residue redissolved in water, acidulated with muriatic acid. A white powder remained, which, after being heated to redness, weighed 16·8 grains, and possessed the properties of silica. Silex.

3. The solution was precipitated by ammonia, and the brown matter, which had separated, boiled for some time Alumine.

* If, as the following analysis would lead us to expect, the specimen examined was a mixture of four parts iserine, and one part quartz and felspar, the specific gravity of pure iserine should be 4·964.

in

in liquid potash. The whole was then thrown on a filter, to separate the undissolved part; and the liquid, which came through, was mixed with a solution of sal ammoniac. A white powder fell, which, after being heated to redness, weighed 3·2 grains. It was alumina.

Iron. 4. The brown substance collected on the filter was dried, drenched in oil, and heated to redness. It was strongly attracted by the magnet, and weighed 52 grains.

5. It was digested in diluted sulphuric acid; but not being rapidly acted upon, a quantity of muriatic acid was added, and the digestion continued. The whole slowly dissolved, except a blackish matter, which became white when exposed to a red heat, and, as far as I could judge from its properties, was oxide of titanium, slightly contaminated with iron. It weighed 1·8 grain.

More oxide of titanium.

6. The acid solution being concentrated by gentle evaporation, a number of small yellowish-coloured needles made their appearance in it. By repeated evaporations, all the crystals that would form were separated. They weighed 6 grains. I redissolved them in water, and added some ammonia to the solution. A fine yellow powder fell, which I soon recognized to be oxide of uranium. It weighed 4·2 grains.

Oxide of uranium.

7. Thus it appears, that the 52 grains (No. 4), attracted by the magnet, contained 46 grains of iron, and 6 grains of uranium and titanium.

Results of the analysis. 8. The following are the substances separated from 100 grains of iserine, by the preceding analysis:

Oxide of titanium	54·8
Oxide of iron	46·0
Oxide of uranium	4·2
Silica	16·8
Alumina	3·2
Total,	125·0

Here is an excess of no less than 25 grains, to be accounted for by oxigen, which must have united to the three metals during the process. As to the silica and alumina, there can be little hesitation in ascribing them to grains of sand, which had been mixed with the ore. The pure iserine, in

all

all probability, was composed of iron, titanium, and uranium. If we suppose that each of these metals existed in the state of protoxide, we must diminish the titanium by one fourth, the iron by one seventh nearly, and the uranium, according to Bucholz's experiments, by one fifth. This would give us,

Titanium, ...	41·1*
Iron, ...	39·4
Uranium, ...	3·4
Silica and alumina, ...	20·0
	103·9

Here, then, is still an excess of nearly 4 *per cent.* But this I am disposed to ascribe to the oxides of titanium and uranium having been only dried upon the steam bath. Upon the whole, it appears, that in the specimens of iserine analysed, the proportions of titanium and iron were nearly equal, and that the uranium did not exceed 4 *per cent.* The appearance of uranium surprised me a good deal. I perceive, however, that it has already been detected in this ore, from an analysis published by Professor Jameson, in the second volume of his Mineralogy, which, I understand, was made by Lampadius. The specimen examined by Lampadius yeilded very nearly 60 parts of titanium, 30 of iron, and ten of uranium. Whereas, in mine, if the foreign matter be removed, there was obtained, very nearly, Component parts.

48 titanium,
48 iron,
4 uranium,

100

But there can be no doubt, that the iserine which I analysed was still contaminated with a good deal of iron sand; for it was impossible to remove the whole.

Analysis of a Gray Copper Ore, from Airthrey.

The copper mine of Airthrey, near Stirling, consists of a thin vein, which runs through the west corner of the Ochils. It has been twice wrought, by two different com- Copper mine of Airthrey.

* This calculation supposes it in the state of deutoxide. As protoxide it would not exceed 35·3. C.

panies,

panies. But, in both cases, was abandoned, after a few years trial. I went to it some years ago, and examined the ore, at the request of one of the proprietors. The specimens, which were employed for the subsequent analysis, were the purest that I could select out of a considerable quantity. I was told, however, that from the lower level, which was at that time full of water, much richer ore had been extracted. But, afterward, when the lower level was freed from its water, I went down to it myself, and found the ore precisely of the same kind as in the upper, with this difference, that it was more mixed with a calcareous spar, and perhaps, on that account, more easily smelted.

Veinstones. The veinstones in the Airthrey mine are sulphate of barytes, and carbonate of lime, and with these the ore is almost always more or less mixed.

The ore described. The colour is at first a light steel gray; but the surface soon tarnishes, and becomes of a dark dull leaden gray, and in some places assumes a beautiful tempered steel tarnish. Massive and disseminated. In some specimens, it exhibits the appearance of imperfect crystals. Internal surface shining and metallic; but, by exposure, it soon becomes dull. Fracture small grained, inclining to even. Fragments indeterminate, and rather blunt edged. Semihard, the degree being almost the same as that of calcareous spar; for these two minerals reciprocally scratch each other. Streak similar, opake, brittle, easily frangible, specific gravity 4·878.

The ore first picked, 1. To free the ore as completely as possible from foreign matter, it was reduced to a coarse powder, and carefully picked. It was then digested in diluted muriatic acid, which dissolved a quantity of carbonate of lime, amounting to 13 *per cent* of the original weight of the ore.

then analysed. 2. Thus purified, it was dried on the steam bath, and 100 grains of it were reduced to a fine powder, and digested in diluted nitric acid, till every thing soluble in that menstruum was taken up. The residue was digested in the same manner in muriatic acid; and when this acid ceased to act, the residue was treated with nitro muriatic acid, till no farther solution could be produced. The insoluble matter was of a white colour; it weighed 6·9 grains, and

and was almost entirely sulphate of barytes. No traces of sulphate of lead, or of oxide of antimony could be detected in it by the blow pipe.

3. The three acid solutions being mixed together, no cloudiness appeared, nor was any change produced; a proof that the ore contained no silver. No silver in it.

4. The solution, being evaporated nearly to dryness, was diluted with water, and precipituted by muriate of barytes. By this means, the sulphuric and arsenic acids, which had been formed during the long continued action of the nitric acid on the ore, and the presence of which had been indicated by reagents, were thrown down; for nitrate of lead, added to the residual liquid, occasioned no precipitate; a proof that no arsenic acid was present.

5. The liquid, thus freed from arsenic acid, was mixed with an excess of ammonia. It assumed a deep blue colour, while a brown matter precipitated. It was separated by the filter, and being dried, drenched in oil, and heated to redness, it was totally attracted by the magnet. It weighed 45·5 grains, and was iron. Iron.

6. The ammoniacal liquid was neutralised by sulphuric acid, and the copper thrown down by means of an iron plate. It weighed 17·2 grains. Copper.

7. To ascertain the quantity of sulphur and arsenic, 100 grains of the purified ore, in the state of a fine powder, were put into the bottom of a coated glass tube, and exposed for two hours to a red heat. When the whole was cold, and the bottom of the tube cut off, the ore was found in a round solid mass, having the metallic lustre, a conchoidal fracture, and the colour and appearance of *variegated copper-ore*. It had lost 16 grains of its weight. Sulphur and arsenic.

8. The upper part of the tube was coated with a yellowish brown substance, like melted sulphur. It weighed 12·6 grains. Thus, there was a loss of 3·4 grains. As the tube was long, this loss can scarcely be ascribed to sulphur driven off. I rather consider it as water. For towards the beginning of the process, drops of water were very perceptible in the tube. Whether this water was a constituent of the ore, or derived from the previous digestion in muriatic acid, cannot be determined. Sulphur.

9. When

9. When the 12·6 grains of yellowish brown matter detached from the tube were digested in hot potash-ley, the whole was dissolved, except a fine blackish powder, which weighed 1 grain, and was arsenic. The dissolved portion I considered as sulphur.

Arsenic.

10. The potash solution, being mixed with nitric acid, 4 grains of sulphur fell. The remaining 7·6 grains must have been converted into sulphuric acid, by the action of the nitric acid. Accordingly, muriate of barytes occasioned a copious precipitate.

11. The 84 grains of roasted ore being reduced to a fine powder, mixed with half their weight of pounded charcoal, and roasted a second time in a glass tube, one grain of sulphur sublimed. But the tube breaking before the roasting had been continued long enough, the process was completed in a crucible. The roasted ore weighed 70 grains.

Component parts of the ore.

12. From the preceding analysis, we learn that the constituents of the Airthrey ore are as follows:

Iron, ...	45.5
Copper, ...	17·2
Arsenic, ...	14·0
Sulphur, ...	12·6
Water, ...	3·4
Foreign bodies, ...	6.9
	99·6
Loss, ...	·4
	100·0

If we suppose the water and the earthy residue to be only accidentally present, then the only essential constituents are the first four, and the ore would be a compound of

Iron, ...	51·0
Copper, ...	19·2
Arsenic, ...	15 7
Sulphur, ...	14·1
	100·0

Klaproth's analyses.

If we compare this analysis with several analyses of gray copper ore, lately published by Klaproth, we shall find, that the constituents are the same in both; but the proportions

tions of the first two ingredients are very nearly reversed. Klaproth obtained from 0·4 to 0·5 of copper, and from 0·22 to 0·27 of iron. This renders it obvious, that the two ores were not in the same state. I have little doubt, that the difference, however, is merely apparent, and that it arose, altogether, from a quantity of iron pyrites, and perhaps also of arsenic pyrites, which I could not separate from the gray copper ore which I examined. Both of these minerals could be distinctly seen in many of the specimens, intimately mixed with the gray copper; and I have no doubt, that the same mixture existed, even in those specimens which were selected as purest. The difference in the proportions of copper and arsenic, obtained by Klaproth* in his various analyses, is so considerable, as to lead to a suspicion, that even his specimens, in all probability, contained a mixture of foreign matter.

IV.

Researches on the Oximuriatic Acid, its Nature and Combinations; and on the Elements of the Muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution. By H. DAVY, *Esq. Sec. R. S. Prof. Chem. R. I. F. R. S. E.*

(Concluded from vol. XXVII, p. 337.)

I SHALL occupy the time of the society for a few minutes only, for the purpose of detailing a few observations connected with the Bakerian lectures delivered in the last two years; particularly those parts of them relating to sulphur and phosphorus, which new and more minute inquiries have enabled me to correct or extend.

I have already mentioned, that there are considerable differences in the results of experiments made on the action Differences in the results of

* Gehlen's Jour. vol. V, p. 9, 11, 13.

of

experiments on sulphur and phosphorus.

of potassium on sulphur and phosphorus, and their combinations with hidrogen, according to different circumstances of the process. I shall now refer to such of these circumstances, as I have been fully able to investigate.

Sulphur contains an acid,

The able researches of Dr. Thompson have shown, that sulphur, in its usual state, contains small quantities of acid matter; and though, in my first experiments, I conceived that by employing crystallized native sulphur, which had been recently sublimed in nitrogen, I should avoid the presence of any foreign matter, yet I am inclined to believe, that this is not the case; for, by subliming some similar sulphur in nitrogen, I find, that litmus paper placed in the upper part of the retort is slightly reddened.

even when sublimed in nitrogen.

Retort must be lined with sulphur when combining with potassium.

When potassium is made to unite with sulphur, if the retort employed is not lined with sulphur, some of the potassium is destroyed by acting upon the glass; and when large quantities of sulphur are used, it is very difficult to decompose the whole of the sulphuret of potassium by an acid: sulphuretted hidrogen likewise is soluble in muriatic acid; and this circumstance led me to underrate the quantity of sulphuretted hidrogen given off in experiments of this kind*.

Sulphuretted hidrogen evolved from it underrated.

Another errour pointed out.

In acting upon sulphuretted hidrogen by potassium in my early experiments, I used large quantities of the gas and of the metal; and in these cases I have reason to believe, that the violence of the combustion occasioned the decomposition of a considerable quantity of the gas; and, in consequence, led me to form erroneous conclusions concerning the nature of this curious operation.

More accurate mode of conducting these

In all late experiments, in which sulphur, or sulphuretted hidrogen was concerned, I have used muriatic acid satu-

* This circumstance has been pointed out by Messrs Gay-Lussac and Thenard, in a paper printed in the Journal de Physique for December, in which these gentlemen endeavour to show, that, whether potassium has been acted upon by large or small quantities of sulphur, and under all circumstances, it evolves a quantity of gas exactly equal to that which it produces by the action of water. I have been able to gain no results so precise on this subject. I have in another place (the same journal in which their memoir has appeared) offered some observations on their inquiries.

rated

rated with sulphuretted hidrogen over mercury. I have employed sulphur distilled from iron pyrites in vacuo, which did not in the slightest degree affect litmus paper, and I have combined it with potassium in retorts of green glass, or plate glass lined with sulphur, and filled with very pure nitrogen or hidrogen. In making potassium act upon sulphuretted hidrogen, I have employed the gas only in the quantities of from 1 to 3 cubical inches, and have made the combination in narrow curved tubes of green glass over dry mercury. With all these precautions, and after having made a great number of experiments, I am not able to gain perfectly uniform results. Yet there is a sufficient correspondence between them, to enable me to form conclusions, which I may venture to say cannot be far from the truth.

experiments. Sulphur sublimed from pyrites in vacuo not acid. Results not perfectly uniform.

When 1 grain of potassium, which would give by the action of water about 1 cubical inch and $\frac{1}{10}$ of hidrogen, is made to act upon about half a grain of sulphur, some sulphur sublimes during the combination, which always takes place with heat and light; and from $\frac{1}{10}$ to $\frac{1}{16}$ of a cubical inch of sulphuretted hidrogen is evolved. The compound acted on by muriatic acid, saturated with sulphuretted hidrogen, affords from $\frac{9}{10}$ to $\frac{11}{10}$ of a cubical inch of pure sulphuretted hidrogen.

Action of potassium on sulphur, in small proportion,

When more sulphur is used, so as to be from twice to ten times the weight of the potassium, the quantity of sulphuretted hidrogen evolved by the action of the acid is from $\frac{7}{10}$ to $\frac{9}{10}$; but if heat be applied to the combination, so as to drive off the superfluous sulphur, the quantity of gas collected is very little inferior to that produced from the combination in which a small proportion of sulphur is used; and I am inclined to believe, from the phenomena presented in a great number of experiments, that sulphur and potassium, when heated together under common circumstances, combine only in one proportion, in which the metal is to the sulphur nearly as 3 to 1 in weight; and in which the quantities are such, that the compound burns into neutral sulphate of potash.

and in larger proportion. They combine only in one proportion.

When a grain of potassium is made to act upon about 1·1 cubical inches of sulphuretted hidrogen, all the hidrogen

Action of potassium on sulphuretted hi-

drogen, in due proportion; is set free, and a sulphuret of potassium, containing one fourth of sulphur, is formed, exactly the same as that produced by the immediate combination of sulphur and the metal.

and in larger proportion. When sulphuretted hidrogen is employed in larger quantities, there is an absorption of this gas, and a volume is taken up about equal to the quantity of hidrogen disengaged; and a compound of sulphuretted hidrogen and sulphuret of potassium is formed, which gives sulphuretted hidrogen by the action of an acid, nearly double in quantity to that given by the sulphuret of potassium.

Potassium combines with phosphorus only in one proportion. From a number of experiments, I am inclined to believe, that potassium and phosphorus, in whatever quantities they are heated together, combine only in one proportion, a grain of potassium requiring about $\frac{1}{4}$ of a grain of phosphorus to form a phosphuret; which, when acted upon by muriatic acid, produces from $\frac{5}{10}$ to $\frac{8}{10}$ of a cubical inch of phosphuretted hidrogen.

Action of potassium on phosphuretted hidrogen. Half a grain of potassium decomposes nearly three cubical inches of phosphuretted hidrogen, and sets free rather more than four cubical inches of hidrogen; and the phosphuret formed seems to be of the same kind as that produced by direct combination of the metal with phosphorus.

Weight of an ultimate atom of sulphur deduced from its compounds. If, according to Mr. Dalton's ideas of proportion, the quantity in which sulphur enters into its combinations were to be deduced from its union with potassium, in which it seems to form about $\frac{1}{4}$ the weight of the compound, the number representing it would be 13·5. I have lately weighed sulphuretted hidrogen, and sulphureous acid gas, with great care: the specific gravity of the first at mean temperature and pressure, from my experiments, is 10645, which differs very little from the estimation of Mr. Kirwan: that of sulphureous acid gas I find is 20967. Sulphuretted hidrogen, as I have shown, contains an equal volume of hidrogen; and on this datum the number representing sulphur is 13·4. I have never been able to burn sulphur in oxigen without forming sulphuric acid in small quantities; but in several experiments I have obtained from 92 to 98 parts of sulphureous acid from 100

Sp. grav. of sulphuretted hidrogen and sulphureous acid gas.

of

of oxigen in volume; from which I am inclined to believe, that sulphureous acid consists of sulphur dissolved in an equal volume of oxigen; which would give the number as 13·7* nearly, considering the acid gas as containing 1 portion of sulphur, and 2 of oxigen; and these estimations do not differ from each other materially.

Weight of an ultimate atom of phosphorus.

I have made several experiments on the combustion of phosphorus in oxigen gas. From the most accurate, I am inclined to conclude that 25 of phosphorus absorb in combustion about 34 of oxigen in weight; and considering phosphoric acid as composed of 3 proportions of oxigen and 1 of phosphorus, the number representing phosphorus will be about 16·5, which is not very remote from the number that may be deduced from the composition of phosphoret of potassium.

Sulphur and phosphorus may contain oxigen and hidrogen combined in them,

The numbers, which represent the proportions in which sulphur and phosphorus unite with other bodies, are such, as do not exclude the existence of combined portions of oxigen and hidrogen in their constitution; but it may be questioned, whether the opinion which I formed, that the inflammable gas disengaged from them by electricity is necessary to the peculiar form in which these bodies exist, is not erroneous. Phosphorus, as I have stated in the last Bakerian lecture, is capable of forming a solid hydruret: and a part of the sulphur distilled from iron pyrites is usually of a soft consistence, and emits the smell of sulphuretted hidrogen, and probably contains that body. It is not unlikely, that in all cases phosphorus and sulphur

and probably in no case perfectly free

* The estimation from the composition of sulphuretted hidrogen must be considered as most accurate, and that from the formation of the sulphuret of potassium as least accurate: for it was only by combining sulphur and potassium in small proportions, and ascertaining in what cases uncombined sulphur could be distilled from the compound, that I gained my conclusions concerning the composition of the sulphuret of potassium.

In the last Bakerian lecture, I have estimated the specific gravity of sulphuretted hidrogen at 35 grains the 100 cubical inches, which was not far from the mean, between the estimations of Mr Kirwan and Mr Thenard. According to this experiment, sulphuretted hidrogen is composed of 1 portion of hidrogen, represented by 1, and 1 of sulphur represented by 13·4.

from their hydrurets.

contain small quantities of the hydrurets of phosphorus and sulphur; and the production of a minute portion of sulphuric acid in the slow combustion of sulphur is probably connected with the production of water. Though the pure oxides of sulphur and phosphorus have never been obtained, yet, from the doctrine of definite proportions, these bodies ought, under certain circumstances, to be formed. And I am inclined to believe, that they sometimes exist in minute quantities, in common phosphorus and sulphur, and, with hidrogen, give to them their variable properties.

Pure oxides of them never yet obtained.

Phosphorus differs in colour, probably from a mixture of oxide.

The colours of different specimens of phosphorus, as well as of sulphur, differ considerably; the red colour of phosphorus, as it is commonly prepared, is probably owing to a slight mixture of oxide. Common roll sulphur is of a very pale yellow; the Sicilian sulphur of an orange colour; and the sulphur distilled from iron pyrites in vacuo, which arose in the last period of the process, of a pale yellowish green colour. All the late experiments that I have made, as well as my former researches, induce me to suspect a notable proportion of oxigen in Sicilian sulphur; which is probably owing to the presence of oxide of sulphur, which may give rise to sulphuric acid in distillation; or to sulphuric acid itself.

So does sulphur.

Action of oximuriatic acid gas

Conceiving, that, if definite proportions of oxigen and hidrogen existed in sulphur and phosphorus, they ought to be manifested in the agency of oximuriatic acid gas on these bodies, I made some experiments on the results of these operations. In the first trial, on the combination of

on sulphur,

sulphur with oximuriatic acid gas, I employed 5 grains of roll sulphur, and admitted the gas into the exhausted retort, from a vessel in which it had been in contact with warm water: in this case more than half a cubical inch of oxigen gas, and nearly two cubical inches of muriatic acid gas, were produced. Suspecting in this instance, that aqueous vapour had been decomposed, I employed cold water in the next experiment, and dried the gas by muriate of lime; in this case, though Sicilian sulphur was used, no oxigen gas was evolved; and not half a cubical inch of muriatic acid; the quantity was the same as in the last experiment;

experiment; and it was found, that between 16 and 17 cubical inches of oximuriatic acid gas disappeared: the whole of the sulphur was sublimed in the gas, and the liquor formed was of a tawny orange colour.

and on phosphorus.

No oxigen was expelled during the combustion of phosphorus in oximuriatic acid gas; nor could I ascertain, that any muriatic acid had been formed. Three grains of phosphorus were entirely converted into sublimate, by the absorption of about 23 cubical inches and a half of the gas.

Proportion of the compounds.

It would seem from these quantities, that the sulphuretted liquid, formed by subliming sulphur in oximuriatic acid gas, consists of 1 proportion of sulphur, represented by 13·5, and 1 of oximuriatic gas represented by 32·9, and that the phosphoric sublimate must be composed of 3 portions of oximuriatic gas, represented by 98·7, and 1 of phosphorus represented by 16·5.

V.

On the Culture of Parsneps, and their Utility in Feeding Cattle: by CHARLES LE HARDY, *Esq., of the Island of Jersey**.

SIR,

Culture of parsneps, in Jersey,

HAVING observed in the book of premiums offered by the society, that they wished for information on the culture of parsneps, which are much used in the island of Jersey; as having practised it for many years, I take the liberty to communicate what I know on the subject, with the result of some comparative experiments.

for hogs and cattle.

The culture of parsneps and beans is looked upon as one of the regular courses of crops in the island. There is no farmer, be the extent of his grounds ever so small, who does not yearly plant a proportionate quantity, for the purpose of fattening his hogs and cattle, or feeding his milch cows.

* Trans. of the Society of Arts, vol. XXVII, p. 52. The silver medal was voted to Major Le Hardy for this communication.

A few

They answer better than potatoes.

A few years ago, the culture of potatoes was substituted by some farmers to that of parsneps, and apparently with advantage; but farther experience has brought them back again to their former practice. Potatoes produce more weight and measure on a given extent of ground, and may be cultivated with less expense; still the parsnep is found to answer best for the farmer's purpose. A perch of the island, which is twenty-four square feet, will produce on an average crop seven cabots of potatoes, each weighing forty pounds, the same extent in parsneps will only average six cabots, which weigh only thirty-five pounds each, making twenty* pounds weight in favour of the potatoes, but they are not so nutritious as parsneps.

Soil.

Mode of culture.

Parsneps will thrive almost any where, but better in a deep stiff loam. They are generally cultivated in the island after a crop of barley, in the following manner.—At the end of January, or the beginning of February, the soil, which requires for this purpose to be stirred from the bottom, is either dug with spades after a skimming plough, or with two ploughs of different shapes following one another. The latter of the two, invented some years ago by a farmer in the island, will go to a depth of fifteen inches. In both these ways, the neighbouring farmers assist each other: In the season, it is not uncommon to see forty or fifty men in one field digging after a plough. When the large plough is used, less men are required, but more strength of cattle: two oxen and six horses are the team generally used. Those days are reckoned days of recreation, and tend to promote social intercourse among that class of men.

Beans dibbled in, and then parsneps sown.

After the ground has been tilled in this way, it is coarsely harrowed, and a sufficient number of women are provided to plant beans. These are dibbled in rows three by three

. . . . . .
. . . . . at the distance of five feet from row to row.
. . . . . .

Two women may plant one vergee in a day. Two vergees and a half being equal to an English acre. Three sextenniers of parsnep seed, (about ¼ of a Winchester bushel) are then sown upon each vergee, and the whole is finely harrowed.

* Seventy, if the preceding numbers be right. C.

This

This crop now requires no attendance till the month of May, when weeding becomes necessary. This is the most expensive part of the culture. It is generally done by hand, with a small weeding fork, and as the parsneps require to be kept very clean, the expense is proportionate to the quantity of weeds. This latter summer four women were employed twenty-eight days each in weeding about five vergees. I tried a few perches with the hand hoe, and thinned them like turnips; they proved finer than those which were hand weeded. In Guernsey they make use of the spade for this purpose.

Weeding, by hand, the hoe, or the spade.

In the beginning of September, the beans are pulled up from among the parsneps, and about the latter end the digging begins. The instrument used is the common three pronged fork. This work is done gradually as the cattle want them, till the ground requires to be cleared for sowing wheat; which after parsneps is generally done about the middle of December. They are reckoned an excellent fallow for that kind of grain, and the finest crops are generally those which succeed them; as it is a tap rooted plant, it does not, like the potato, impoverish the surface, but leaves it mellow, and free from weeds, to a succeeding crop.

Digging up. Followed by wheat, for which they are an excellent preparation.

When parsneps require to be kept for the use of cattle, they are brought under dry sheds, and will keep good without any care till the end of March. Should they require to be kept longer, they are laid in double rows over one another, their heads outward, with alternate strata of earth, which, when finished, have the appearance of small walls, or, if made circular, of small towers. Those for seed are always preserved in this manner, and sometimes carrots and beets for culinary purposes.

Mode of keeping them.

Parsneps are not injured by frost; after having been frozen, they are fit for vegetation; the only sensible alteration is their acquiring a sweeter taste, and by this perhaps becoming more nutritive. They are given raw to hogs and to horned cattle. Though horses are fond of these roots, they are not suffered to eat them, as they make them languid, and are apt to injure their sight. Their leaves, when

Not injured by frost. Injurious to horses.

The wet leaves caustic. when wet, are so caustic as to blister the hands of the weeders, and sometimes to occasion a violent inflammation in the eyes and udders of the cattle feeding upon them.

Good food for milch cows. Cows fed on parsneps in the winter months give a greater quantity of milk and butter, and of better flavour, than those fed upon potatoes. The butter is nearly equal to that from spring grass. Though the root of this plant has the quality of improving that article, it must be observed, that the leaves give it a very disagreeable taste, which however is of no consequence when intended to be potted, as it goes off in a short time.

Bad for breeding sows, but good to fatten hogs, Parsneps are dangerous food for sows before they farrow, and might occasion them to lose their litter. Hogs may be fattened with them in about six weeks. It is the custom during that time, to thicken their swill with the meal of beans and oats ground together. Pork fattened in this way is very firm, and does not waste in boiling.

or horned cattle, Horned cattle may be fattened with parsneps in about three months. I never knew them used for sheep.

which are much improved by them. It is the general opinion in the island, that hogs or cattle fed on parsneps may be brought to a condition for slaughtering, in less time, and with half the quantity, that would be required of potatoes. The butchers are sensible of the superiority of the former, and will give a halfpenny per pound more for cattle fattened with them, than for such as have been fed any other way. Upon inquiry I was informed, they always contained a greater quantity of tallow.

This I believe to be a full account of the culture and use of the parsnep, and a just comparison with the potato.

I remain, Sir,

Your most obedient humble servant,

CHARLES LE HARDY.

VI.

VI.

Observations upon Luminous Animals. *By* J. MACARTNEY, *Esq.* *Communicated by* EVERARD HOME, *Esq. F. R. S.*

(Concluded from vol. XXVII, p. 350.*)*

THE remarkable property of emitting light during life is only met with among animals of the four last classes of modern naturalists, viz. mollusca, insects, worms, and zoophytes. Only the 4 last classes of animals emit light while living.

The mollusca and worms contain each but a single luminous species; the *pholus dactylus* in the one, and the *nereis noctiluca* in the other. Only 2 luminous species in the 1st and 3d.

Some species yield light in the eight following genera of insects; *elater*, *lampyris*, *fulgora*, *pausus*, *scolopendra*, *cancer*, *lynceus**, and *limulus*. The luminous species of the genera lampyris and fulgora are more numerous than is generally supposed, if we may judge from the appearance of luminous organs to be seen in dried specimens. Genera of insects containing luminous species.

Among zoophytes we find, that the genera *medusa*, *beroe*†, and *pennatula*, contain species which afford light. Genera of zoophytes.

The only animals, which appear to possess a distinct organization for the production of light, are the luminous species of lampyris, elater, fulgora, and pausus. Animals with organs for producing light.

The light of the lampyrides is known to proceed from some of the last rings of the abdomen, which, when not illuminated, are of a pale yellow colour. Upon the internal surface of these rings there is spread a layer of a peculiar soft yellow substance, which has been compared to paste; but by examination with a lens I found it to be organized like the common interstitial substance of the insect's body, except that it is of a closer texture, and a Organ in the fireflies. A peculiar substance,

* The animal discovered by Riville off the coast of Malabar, in 1755, is certainly a testaceous insect, and appears to belong to the genus lynceus of Muller.

† The luminous zoophite, for which Peron has instituted the new genus pyrosoma, appears to me to be a beroe, and only worthy of a specific distinction.

paler

paler yellow colour. This substance does not entirely cover the inner surface of the rings, being more or less deficient along their edges, where it presents an irregular waving outline. I have observed in the glow-worm, that it is absorbed, and its place supplied by common interstitial substance, after the season for giving light is past.

existing at particular seasons.

The segments of the abdomen, behind which this peculiar substance is situate, are thin and transparent, in order to expose the internal illumination.

The number of luminous rings varies in different species of lampyris; and, as it would seem, at different periods in the same individual.

Two sacs in the glow-worm,

Beside the luminous substance above described, I have discovered in the common glow-worm, on the inner side of the last abdominal ring, two bodies, which to the naked eye appear more minute than the head of the smallest pin. They are lodged in two slight depressions, formed in the shell of the ring, which is at these points particularly transparent. On examining these bodies under the microscope, I found, that they were sacks, containing a soft yellow substance, of a more close and homogeneous texture, than that which lines the inner surface of the rings. The membrane forming the sacs appeared to be of two layers, each of which is composed of a transparent silvery fibre, in the same manner as the internal membrane of respiratory tubes of insects; except that in this case the fibre passes in a spiral, instead of a circular direction. This membrane, although so delicately constructed, is so elastic as to preserve its form, after the sac is ruptured, and the contents discharged.

which emit light almost constantly.

The light, that proceeds from these sacs, is less under the control of the insect, than that of the luminous substance spread on the rings: it is rarely ever entirely extinguished in the season that the glow-worm gives light, even during the day: and when all the other rings are dark, these sacs often shine brightly.

Said to vary in number,

The circumstance of there being points, which give a more permanent light than the other parts of the luminous rings of the abdomen, has been noticed before by the Comte

Comte G. de Razoumouski. He states the number of these luminous points to vary from 2 to 5*.

I must however remark, that I never saw more than two of these luminous points, which were always upon the last ring of the body: and that the figures, which accompany the memoir of the Comte de Razoumouski, bear scarcely any resemblance to the insect they are intended to represent, from which we may fairly suspect him of inaccuracy in other particulars. but probably this inaccurate.

As far as my observation has extended, the small sacs of luminous substance are not found in any species of lampyris, except the glow-worm of this country. Thunberg mentions, that the lampyris japonica has two vesicles on the tail, which afford light. They are not common to the genus.

The organs for the production of light in the genus elater are situate in the corselet. These likewise consist of a peculiar yellow substance, placed behind transparent parts of the shell, which suffer the natural colour of this substance to be seen through them in the day, and, when illuminated, give passage to the light. Organ in the springing beetles.

On dissecting the organs of light in the elater noctilucus, I found that there is a soft yellow substance, of an oval figure, lodged in the concavity of the yellow spots of the corselet, which parts are particularly thin and transparent in this species. This substance is so remarkably close in its structure that at first view it appears like an inorganic mass; but with a lens it is readily perceived to be composed of a great number of very minute parts or lobules closely pressed together. Around these oval masses the interstitial substance of the corselet is arranged in a radiated manner; and the portion of the shell, that immediately covers the irradiated substance, is in a certain degree transparent, but less so than that which lies over the oval masses; it is therefore probable, that the interstitial substance in this situation may be endowed with the property of shining. A fasciculus of the muscles of the corselet arises in the interior of the oval masses of the luminous substance, but not apparently with any design; as it contributes, with the adjacent fasciculi, to move the anterior feet.

* Mem. de la Soc. de Lausanne, Tom. ii.

In

In the elater ignitus, the masses of luminous substance are extremely irregular in their figure: they are situate nearly at the posterior angles of the corselet, and are more loose in their texture than the oval masses of the noctilucus, resembling rather in composition the interstitial substance, which surrounds these masses in that species. The shell of the corselet is somewhat thinner, and more transparent along both sides of the margin, than at other places; but it is not, as in the noctilucus, elevated, and peculiarly clear and thin, immediately over the seat of the luminous organ; consequently the light emitted by the elater ignitus cannot be very brilliant.

I have not been able to procure any specimen of the elater phosphorea: but from the accounts of naturalists it appears to resemble in every respect the elater noctilucus; indeed I have great doubts of the phosphorea being a distinct species.

Organ in the lanternfly.

I have had an opportunity of examining, preserved in a moist way, two species of fulgora, the candelaria and lanternaria. The light in this genus has been observed to issue from the remarkable proboscis of the fore part of the head. This part has always been described by authors as hollow, or empty, which I have found to be perfectly correct; and what is more extraordinary, that the cavity communicates freely with the external air, by means of a chink or narrow aperture, placed on each side of the root of the proboscis. This projection is covered internally by a membrane, between which and the horny part, or shell, there appears to be interposed a pale reddish coloured soft substance, that is arranged in the candelaria in broad lines or stripes; but it is so thin, that I could not distinctly examine its structure, or absolutely determine, whether it should be considered as a substance intended to furnish the light of these insects, or the pigment upon which the colour of the proboscis depends.

Pausus spherocerus.

The globes of the antennæ constitute the organs of light in the pausus spherocerus. Dr. Afzelius, who discovered the luminous property in this species, compares them to lanterns spreading a dim phosphoric light*. The rarity

* Lin. Trans. Vol. IV.

of

of the insect put it out of my power to examine its structure, but from the form and situation of its organs of light, it is most probable they are constructed like those of the fulgorae.

It has been conjectured by Carradori and others, that the lampyrides were enabled to moderate or extinguish their light, by retracting the luminous substance under a membrane; but neither in them, or any of the other luminous insects, have I found an apparatus of this sort. The substance furnishing the light is uniformly applied to corresponding transparent parts of the shell of the insect, whence it is not moved; indeed a membrane, if it did exist, would have but little effect in obscuring the light, and never could serve to extinguish it. The regulation of the kind and degree of the luminous appearance does not depend upon any visible mechanism; but, like the production of the light itself, is accomplished by some inscrutable change in the luminous matter, which in some animals is a simple operation of organic life, and in others is subject to the will.

The light regulated not by any visible mechanism,

but by some change in the luminous matter.

It is worthy of remark, that, in all the dissections I have made of luminous insects, I did not find; that the organs of light were better, or differently supplied with either nerves or air tubes, than the other parts of the body. The power of emitting light likewise exists in many creatures which want nerves, a circumstance strongly marking a difference between animal light, and animal electricity.

The light of insects unconnected with nervous energy.

With the exception of the animals above mentioned, the exhibition of light depends upon the presence of a fluid matter.

In other animals the luminous matter is fluid:

In the pholas dactylus, the luminous fluid is particularly evident, and in vast quantity; it is recorded by Pliny, that this fluid is like liquid phosphorus, and renders every object luminous, with which it comes into contact. Reaumur also found, that it was diffusible in water, or any other fluid, in which the animal might be immersed*.

as in the pholas dactylus;

The shining of the scolopendra electrica I have always observed to be accompanied by the appearance of an effu-

scolopendra electrica;

* Mém. de l'Acad. des Sc. 1712.

sion

sion of a luminous fluid upon the surface of the animal, more particularly about the head; which may be received upon the hand, or other bodies brought into contact with the insect at the moment; and these exhibit a phosphoric light for a few seconds afterward. This fluid, however, I never could discover in the form of moisture, even upon the clearest glass, although examined immediately with the most scrupulous attention by a lens: it must therefore be extremely attenuated.

nereis noctiluca; The same appearance has been observed during the illumination of the nereis noctiluca by Fougeroux de Bondaroy*.

and others. The animal discovered by Riville shed a blue liquor, which illuminated the water for a distance of two or three lines†.

Spallanzani relates, that the medusa, which he examined, communicated the property of shining to water, milk, and other fluids, on being rubbed or squeezed in them‡.

This fluid partial, or general. The luminous fluid is in some instances confined to particular parts of the body, and in others is diffused throughout the whole substance of the animal.

In the scolopendra electrica, it appears to reside immediately under the integuments. In the lynceus discovered by Riville it is contained in the ovary. If I may judge from my own observations, every part of the body of the medusæ is furnished with this fluid, as there is no part I have not seen illuminated under different circumstances; but Spallanzani affirms, that it is only found in the large tentacula, the edges of the umbella, and the purse, or central mass; which he proved, he says, by detaching these parts successively, when they shone vividly, while the rest of the body neither gave light, nor communicated any luminous appearance to water§.

Reapen. Spallanzani discovered a mucous luminous fluid in the plumule of the pennatula phosphorea‖.

* Mém. de l'Acad. des Sc. 1767.

† Mém. Etrang. de l'Acad. des Sc. Tom. iii.

‡ Spallanzani's Travels in the Two Sicilies, Vol. iv.

§ Memoria sopra le meduse fosforiche. Mem. della Soc. Ital. Tomo vii.

‖ Mem. della Soc. Ital. Tomo ii.

The

The phenomenon of animal light has been attempted to be explained in different ways. By many persons it was formerly ascribed to a putrefactive process; but, since the modern theories of combustion became known, it has been generally believed to depend upon an actual inflammation of the lumous substance, similar to the slow combustion of phosphorus. Others have accounted for the luminous effect, by supposing the matter of light to be accumulated, and rendered latent under particular circumstances, and afterward evolved in a sensible form. Theories of animal light.

The opinion of the light of living animals being the consequence of putrefaction is evidently absurd, and contradictory to all observation on the subject. It has been proved by the experiments of Dr. Hulme and others, that even the luminous appearances of dead animals are exhibited only during the first stages of the dissolution of the body, and that no light is emitted after putrefaction has really commenced. It cannot be owing to putrefaction.

Spallanzani, who was the most strenuous advocate for the phosphorescent nature of animal light, stated, that glow-worms shone more brilliantly when put into oxigen gas; that their light gradually disappeared in hidrogen or in azotic gas, and was instantly extinguished in fixed air; that it was also lost by cold, and revived by the application of a warm temperature. He conjectured, that the luminous matter of these insects was composed of hidrogen and carbonated hidrogen gas. Arguments for its being phosphorescent by Spallanzani

Forster relates, in the Lichtenberg Magazine for 1783, that, on putting a lampyris splendidula into oxigen gas, it gave as much light as four of the same species in common air. and Forster.

Carradori has made some experiments upon the lucciole, (lampyris italica) which led him to deny its phosphorescence. He found, that the luminous portion of the belly of the insect shone in vacuum, in oil, in water, and different liquids, and under different circumstances, where it was excluded from all communication with oxigen gas. He accounts for the result of Forster's experiment, by supposing, that the worm shone more vividly, because it was more animated in oxigen gas than in common air. Arguments against it by Carradori.

Carradori

Supposed by im and Brugnatelli to be the matter of light.

Carradori adopts on this subject the doctrine of Brugnatelli, and ascribes the luminous appearances of animals to the condensation and extrication of light in particular organs, which had previously existed in combination with the substance of their bodies. He supposes the light to be originally derived from the food, or the atmospheric air taken into the body: in short, that certain animals have the peculiar property of gradually imbibing light from foreign bodies, and of afterward secreting it in a sensible form *.

Experiments by the author.

The following experiments, which I made upon this subject, would lead me to make different conclusions than those of the preceding authors.

Glow-worm in water luminous.

Exp. 1. A glow-worm was put into a glass of water, in which it lived nearly two hours, and continued to emit light as usual, until it died, when the luminous appearance entirely ceased.

The substance not luminous from dead,

Exp. 2. The luminous substance was extracted from the beforementioned glow-worm, and from others killed in different ways; but it afforded no light.

but luminous when taken from living animals.

Exp. 3. The sacs containing the luminous matter were cut from the bellies of *living* glow-worms, and shone uninterruptedly for several hours in the atmosphere; and after their light became extinct, it was revived by being moistened with water. Some of these were put into water in the first instance, in which they continued to shine unremittingly for 48 hours.

Not luminous by heat.

Exp. 4. The luminous substance of a glow-worm was exposed to a degree of heat, which would have been sufficient to inflame phosphorus, without increasing the brilliancy of its light; and farther, it could not be made to burn by being applied to a red hot iron, or to the flame of a candle.

A little heat apparently evolved during the shining of glow-worms,

Exp. 5. A delicate thermometer was introduced among some living glow-worms, during the time they gave out much light: the temperature of the room being 69, the instrument rose to 75, 76, and 77, according to circumstances, as the warmth was reflected from the hand, or dis-

* Annal di Chimica, Tomo xiii. 1797.

sipated

sipated by the worm crawling over cold substances. The luminous portion of the tail, when very brilliant, appeared to raise the thermometer more quickly than the other parts of the body, but it was not invariably the case. When shining strongly, I thought that the luminous rings communicated the sensation of warmth to the hand, but this was probably a deception, as the actual degree of heat was not sufficient for such an effect. It should however be mentioned, that in Templar's observations on the glow-worm, he said his feelings deceived him, if he did not experience some heat from the shining of the insect*.

but only while they were alive.

Exp. 6. To satisfy myself how far the evolution of heat during the shining of glow-worms depended upon the life of the animals, I cut off the luminous portion of the tail from several living worms; and I found, that, if the thermometer was applied to them immediately, it was raised by them one or two degrees; but after these parts were dead, although they continued to emit light, they produced no effect whatever upon the instrument.

Medusæ shone when heated in sea water,

Exp. 7. Some hemispherical medusæ were put into a spoon, containing a small quantity of seawater, and held over a burning candle. As soon as the water became heated, the medusæ appeared like illuminated wheels, the spots at the margin and centre alone emitting light; in which manner they shone vividly and permanently for about 20 seconds, when they shrunk and died, after which they were no longer luminous.

immersed in spirits,

Exp. 8. Some of the same species were put into spirits: a strong and unremitting light was instantly given out, which issued from the central and marginal parts, as in the preceding experiment, and continued until they died.

or shaken in a vacuum:

Exp. 9. Some of the scintillating and hemispherical species of medusa, contained in a small glass jar, were introduced into the receiver of an air pump, and, the air being exhausted, they shone as usual when shaken; if any difference could be perceived, the light was more easily excited, and continued longer in vacuo.

* Phil. Trans. No. 72.

I wished next to try the influence of electricity on the luminous property of animals.

but not when electrified,

Exp. 10. A medusa hemispherica was placed in a small glass dish, containing a quantity of water, merely sufficient to allow the animal to preserve its figure; being insulated, it was electrified, and sparks drawn from it, which had not the slightest effect; the experiment was repeated several times with different individuals, but without exciting the animals to throw out light.

unless when slight shocks were passed through them.

Exp. 11. Some hemispherical medusæ were placed in contact with the two ends of an interrupted chain, and slight electric shocks passed through them. During the very moment of their receiving the shock no light was visible, but immediately after the medusæ shone like illuminated wheels, which appearance remained for some seconds. Upon the closest inspection with a magnifying glass, no contractile motion could be perceived to accompany the exhibition of the light. The application of electricity in this instance seems to have acted merely as a strong mechanic shock.

The above experiments on the luminous medusæ were made at Herne, with the assistance of George May, Esq., of Stroudhouse, and in the presence of a large company, capable of accurately distinguishing their results.

General deductions from these experiments.

It seems proved by the foregoing experiments, that, so far from the luminous substance being of a phosphorescent nature, it sometimes shows the strongest and most constant light, when excluded from oxigen gas; that it in no circumstances undergoes any process like combustion, but is actually incapable of being inflamed; that the increase of heat, during the shining of glow-worms, is an accompaniment, and not an effect of the phenomenon, and depends upon the excited state of the insect; and lastly, that heat and electricity increase the exhibition of light, merely by operating like other stimuli upon the vital properties of the animal.

The light not affected by any kind of gas.

In confirmation of these opinions, I may quote the high authority of the secretary of the society, who has found, that the light of the glow-worm is not rendered more brilliant in oxigen, or in oxigenated muriatic gas, than in common

common air; and that it is not sensibly diminished in hidrogen gas.

I may farther add, that Spallanzani's experiments of diffusing the luminous liquor of the medusa in water, milk, and other fluids, are in direct contradiction of his own theory; as is also the extinction of the light of these mixtures by the application of a high degree of heat.

Spallanzani's experiments against his own theory.

If the light emitted by animals were derived from their food, or the air they respire, as supposed by Carradori, the phenomenon should be increased or diminished, according to the quantity of food or air, that the creatures consume; but we do not find this to be the case; for in those situations where they are sometimes found to be the most luminous, they are deprived, in a great measure, of these assumed sources of their light.

Their light not derived from food or air,

In fact, the luminous exhibitions of living animals are not only independent of all foreign light, but are frequently destroyed by the latter. I have always found the shining of the medusæ to cease upon the rising of the moon, or at the approach of day; and when out of the sea, I never could excite them to throw out light, until they had been kept for some time in the dark; all the luminous insects likewise secrete themselves as much as possible during the day time, and go abroad only at night. I have, it is true, found, that the scolopendra electrica will not shine, unless it has been previously exposed to solar light; but I have observed, that it shone as brilliantly and as frequently, after being kept a short time in a light situation, as when left uncovered the whole day. The circumstance of the scolopendra requiring exposure previous to its giving out light is very unaccountable; as the insect, when left to itself, always seeks as much as possible concealment during the day; indeed it is the opinion of some naturalists, that it is killed by the light of the sun.

and independent of foreign light,

except perhaps in the scolopendra.

This exception a curious fact.

The opinions of Brugnatelli and Carradori are connected with some general doctrines, respecting the nature of light, which I shall not at present venture to discuss. It appears to me, that the question is still unresolved, whether light has a substantial existence, or is a phenomenon depending upon certain operations or conditions of

Whether light be material not yet determined:

the ordinary forms of matter. But the highly ingenious researches of Count Rumford, on the laws of what have been called subtile fluids; and the extraordinary advances lately made by Mr. Davy, on the decomposition of substances, that were hitherto looked upon as elementary; give us reason to hope, that future investigations may unfold views of the material world, of which we can at present have only an indistinct conception; that new modes of analysis may enable us to see things, not "through a glass darkly," but more nearly as they are; and that the boundaries of physical and metaphysical science, now so far asunder, may be made to approach each other.

In the present state of our knowledge, our business should be, to collect, arrange, and compare phenomena, rather than to speculate upon their nature. Nevertheless, I cannot refrain from observing, that the circumstances attending the luminous appearance of living animals are much more favourable to the supposition of light being a property, than a substance. The quantity of light emitted by an animal in a certain time (admitting it to be matter) far exceeds that which could be possibly supplied by the sources, whence it is usually supposed to be derived. Thus the luminous appearance of some medusæ may be continued with the intermission of short intervals for an indefinite time, notwithstanding the creature be kept in darkness, and without any other food than what a small quantity of filtered seawater would afford. The uninterrupted and long continued light, that is sometimes evolved by the luminous sacs and the ova of the glow-worm, is also inconsistent with the notion of an accumulation and subsequest dispersion of a material substance.

but the phenomena of luminous animals are rather more favourable to the supposition of its being a quality.

I shall terminate this paper by an enumeration of the several conclusions, that are the result of the observations I have been able to make upon the phenomena of animal light.

General conclusions.

The property of emitting light is confined to animals of the simplest organization, the greater number of which are inhabitants of the sea.—The luminous property is not constant, but, in general, exists only at certain periods, and in particular states of the animal's body.—The power of showing

showing light resides in a peculiar substance or fluid, which is sometimes situate in a particular organ, and at others diffused throughout the animal's body.—The light is differently regulated, when the luminous matter exists in the living body, and when it is abstracted from it. In the first case, it is intermitting, or alternated with periods of darkness; is commonly produced or increased by a muscular effort; and is sometimes absolutely dependant upon the will of the animal. In the second case, the luminous appearance is usually permanent until it becomes extinct, after which it may be restored directly by friction, concussion, and the application of warmth; which last causes, operate on the luminous matter (while in the living body,) only indirectly, by exciting the animal.—The luminous matter, in all situations, so far from possessing phosphoric properties, is incombustible, and loses the quality of emitting light, by being dried, or much heated. The exhibition of light, however long it may be continued, causes no diminution of the bulk of the luminous matter. It does not require the presence of pure air, and is not extinguished by other gasses.

The luminous appearance of living animals is not exhausted by long continuance, or frequent repetitions, nor accumulated by exposure to natural light; it is, therefore, not dependent upon any foreign source, but inheres as a property, in a peculiarly organized animal substance or fluid, and is regulated by the same laws, which govern all the other functions of living beings.

Luminousness of the sea.

The light of the sea is always produced by living animals, and most frequently by the presence of the medusa scintillans. When great numbers of this species approach the surface, they sometimes coalesce together, and cause that snowy or milky appearance of the sea, which is so alarming to navigators. These animals, when congregated on the surface of the water, can produce a flash of light, somewhat like an electric corruscation. When the luminous medusæ are very numerous, as frequently happens in confined bays, they form a considerable portion of the mass of the sea, at which times they render the water heavier

heavier, and more nauseous to the taste; it is therefore advisable always to strain seawater before it is drunk.

The luminous property does not appear to have any connection with the economy of the animals that possess it, except in the flying insects, which by this means discover each other at night, for the purpose of sexual congress.

Explanation of the Figures.

Explanation of the plates.

Fig. 1*. The cancer fulgens, discovered by the Right Hon. Sir Joseph Banks, of the natural size.

Fig. 2. The same animal magnified.

Fig. 3. The medusa pellucens, also found by Sir Joseph Banks, represented of the natural magnitude.

Fig. 4. The limulus noctilucus, discovered by Captain Horsburgh, considerably enlarged.

Fig. 5. The luminous medusa, discovered by me, which I conceive to be the medusa hemispherica: it is shown of the largest size I met with.

Fig. 6. The central process of this animal's body magnified, in order to explain its structure. The thick tentacula in which it terminates are seen covered with small cups or suckers.

Fig. 7. The beroe fulgens, discovered by me, shown in its most elongated or relaxed form, which it assumes commonly when swimming quickly.

Fig. 8. The same animal in the most contracted form.

Fig. 9. The minute species of medusa, discovered by me, which is the most frequent cause of the luminous appearance of the sea, represented of the natural size.

Fig. 10. The same animal magnified, exhibiting a puckered or tucked-in appearance on one side.

Fig. 11. Is the animalcule discovered by Forster, of the natural size.

Fig. 12. The same, greatly magnified, to show the intestinal parts. Both these figures are copied from the original drawings, in the possession of the Right Hon. Sir Joseph Banks.

* Figs 1, 2, and 3, will be found in plate IX of the preceding volume, the rest in plate II of this.

Fig. 13.

Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.

Fig. 13.

Fig. 9.

Fig. 10.

Fig. 11.

Fig. 12.

Fig. 14.

Fig. 15.

Fig. 16.

Fig. 17.

Fig. 18.

Fig. 19.

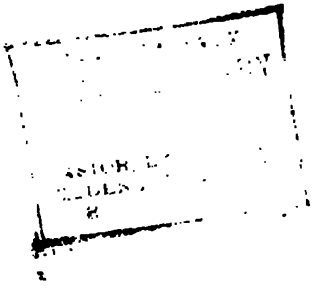


Fig. 13. Is an enlarged view of the inferior surface of the abdomen of the lampyris lucida, after the integument had been removed. *a a a* represent the three masses of luminous substance, which are applied to the three last rings of the abdomen. *b b b* the arrangement of the cellular or interstitial substance on the other abdominal rings, which gives the pale colour to the whole belly of this insect.

Fig. 14. Represents the common glow-worm, with the posterior portion on the back cut away to expose the sacs of luminous matter in situ on the last ring of the belly. *a* indicates the sac of one side; the intestine is seen to lie between them.

Fig. 15 and 16. Are the sacs of the glow-worm prodigiously magnified to show their structure. Fig. 16 is cut open to expose the luminous matter it contains: the coat of the sac is still seen to preserve its figure.

Fig. 17. Is the elater noctilucus, with the shell of the corselet removed on one side, by which the organ of light is uncovered. *a* the yellow transparent spot of the corselet. *b* the oval mass of luminous substance surrounded by an irradiation of the interstitial substance. *c* the ends of the muscles which were on the inside of the corselet.

Fig. 18. Is the posterior angle of the corselet of the elater noctilucus magnified. *a* the radiated appearance which the interstitial substance has round the oval mass of luminous matter. This mass is seen to consist of a number of smaller parts. *b* shows the appearance of the interstitial substance, where it passes down between the muscles. *c* the ends of the muscles of the back. *d* the shell of the corselet.

Fig. 19. Represents the elater ignitus. *a* is the mass of luminous substance of one side, seen indistinctly through the back of the semitransparent portion of the corselet. *b* is the luminous mass of the other side, exposed by removing a part of the shell of the corselet.

VII.

Hints on various Modes of Printing from Autographs. By G. CUMBERLAND, *Esq. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

AS your Journal is frequently enlivened by hints of improvement in the Arts, no less than by accounts of new inventions; allow me, when it is agreeable to you, to occupy a page or two with some thoughts on a subject particularly interesting to authors; and which might, if reduced to practice successfully, be of general service to mankind. Every original writer justly laments the expense, difficulty, and fraud, he is subject to, if he gives his works to the press. If therefore any method could be devised, to enable a man of talents to be his own printer, and take off his own copies as they were demanded, without the intervention of a publisher, a new and brilliant era in the world of letters would be commenced; that would make thought and reflection, when justly employed, as valuable to the possessor as the talent for manual arts; and we might hope to see the day arrive, when the profession of letters might afford as probable a means of getting a fortune as any other profession whatever.

A mode of printing their own works would be very advantageous to men of letters.

In order to accomplish this desirable end, I have, long ago, run over in comparison every substance, that might possibly contribute to this purpose. Metals, wood, clay, paper, have not been forgotten; and although I cannot offer any one as having been fairly tried, (owing to a variety of other occupations), yet I trust I may, by enumerating them, afford hints to others, that may ultimately be useful.

Materials suggested.

My first idea was copper, written on by a stile through white wax, and when printed perused backwards by means of a mirror. *But this would have demanded the talents of a *Blake*, who alone excels in that art: or we must have a

Copper.

* I apprehend there is an omission here of some such words as the following. "It might be written upon backward,"

child

child educated so to write with correctness. Besides, copper is too dear ever to become useful. Yet this method might do for short pieces, if furnished with Dr. *Lind's* press, (a useful instrument, that I believe has never yet been described or published), and I have practised it, but is too costly to answer for large works. Let us therefore suppose a kind of copper or brass latten to be rolled thin for the purpose, and the writer to use a very corrosive ink, which in a short time would eat quite through the whole body. He would by this means produce a stencil as fast as he could write, by means of which he would be enabled to print the right way. Stencilling.

Again let us suppose he were to make use of capital letters only, acting as punches on paper, he would by this method have a paper stencil, that would last as long, perhaps longer, than the latten one. Paper stencil.

Tin foil again might be used in this way, or fine plates of bismuth, for common ingenuity might overcome the difficulties of the O and other letters by ties. Many people will smile at the idea of a paper stencil, who are ignorant of the nature of paper when oiled; but I have been witness to Dr. Lind, of Windsor, printing from a single profile of the King, cut in paper, after he had used it for many thousand impressions, and saw that it was still unimpaired. Tinfoil, or bismuth. Many thousand impressions may be taken from an oiled paper stencil.

Again let us imagine thin and cheap pannels of wood dipped in fine plaster of Paris so as to coat both sides, and then smoothed and well hardened by oil: would not such a block afford good impressions, if we were to cut down its surface, and leave the relief in cameo? This I have tried in blocks of plaster of Paris, and made clear and good impressions from a drawing thus cut out of a small block; which, if not so hard as wood, is still hard enough to impress with, and, where few impressions only are wanted, a very good substitute among men who can make their own drawings, especially where they are objects that do not require effect. Wood covered with plaster of Paris, and cut in relief.

I have also imagined, that for diagrams, or plans, very thick paper firmly glued to a smooth board would, when the interior of the design was cut away, give relief enough for printing, provided the lines were close enough; but in printing Paper glued on wood and cut down.

printing such things as this, we ought to apply the paper extended quite straight, and strike it down by a blow from a flat smooth instrument of the size of the block.

Pontipool ware.

On a Pontipool teaboard I can with a common stile or dry point at any time, with as much ease as I can draw, make an etching where not much force is required. By the same rule I can write on it, and use it as an engraved plate. The paper teaboards might perhaps even be found more useful.

Easy to engrave on pewter, or on copper slightly coated with white wax.

On pewter, made very soft, it is easy to engrave, but very few people know, that on copper also it is as easy to draw with an iron stile, provided you prepare your ground of common white wax spread over it with a dabber when the plate is hot. This thin coat of white wax is useful to receive a pencil tracing of your drawing, which may be deposited on it by a slight friction applied to the back of the paper, or if large by the rolling press; and also serves effectually to prevent the dry point, or stile, from slipping when you begin to scratch the copper through the waxen coat. The burs you may take off by the common mode, or by charcoal partially if you want effect.

Pencil drawings easily engraved thus.

But to return from this not quite a digression, (for if we can learn to write backwards this dry point graving, through white wax, would be the best method of executing plates to imitate writing, and is I believe now used by writing engravers), let us consider cheapness of block as an absolute desideratum; and think whether, if we can find a material that resists the effect of flame, we may not write with that material, and char the rest a little way in, so as to leave a cameo letter,—and if such a plan could be executed, poplar and beech might by machinery be soon reduced to level and cheap blocks. You will perhaps yourself smile when I tell you, that I have used blocks (if they may be so called), of pieces of carrot, to impress mathematical figures of solid forms for the purpose of facilitating instruction; and that a pack of cards were thus made by a son of mine on an emergency, using indian ink instead of oil colour.

Incombustible ink on wood.

Blocks of carrot.

A seal for prints or drawings not easily imitated.

To stamp prints, drawings, or such sort of property, a volute shell ground down on a hone makes the securest seal in the world; for two cannot be made exactly alike. Sections of shells are indeed the best seals we can have, if we wish

wish to avoid imitation. Pearl shell and ivory are however too dear to bring them even into use as plates, and the same objection in a less degree lies against box wood. Glass, by Mr. Walker's method, decomposed by the Swedish acid may, we all know, be easily used as a substitute for both plates and blocks; but it must first be ground flat, and that expense puts an end to its utility. The soft turtle-shell might perhaps, when veneered on wood, be found to answer the purpose of printing—but this again will depend on its abundance. Silver also can be delicately destroyed by quick silver so as to make a block for printing, the form of the letters being first traced in varnish or thick oil; but this would only do for ciphers, or small heads or tokens, for obvious reasons. I have also thought, that, if we could write on a block of stone or plate of glass with an ink so thick as to leave the words in relief, we might, by pressing putty on it, take a cast sufficiently durable to make many impressions; or thus cast it in plaster of Paris from the relievo on the smooth block, but this would print the words white on a black ground; perhaps as good or better than black on white, as less dazzling.

Etching on glass.

Other materials.

Writing in relief.

There are other ideas, but these are enough I trust to rouse our thoughts on this important subject, and lest I tire you and your readers, I conclude by assuring you, that although I have not the honour to be known to you, I am always,

Yours, &c.

G. CUMBERLAND.

Bristol, Nov. 17, 1810.

VIII.

On the Classification of Chemical Agents. By MARSHALL HALL, *Esq. F. R. M. S. E. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

IN your Journal for November, you have given insertion to a small communication from me, "On the Combinations of

of Oxigen;" I now transmit to you an application of the principle there proposed to the classification of chemical agents. I shall first present in order the arrangement, which I beg leave to suggest; and afterward, by way of elucidation, will add a few apposite and necessary observations.

Classification.

Classification of chemical agents.

Class I Attraction.
Class II Repulsion.

as producing a varied state of these.

1 Heat,
2 Light,
3 Chemical rays of the sun,
4 Electricity,
5 Galvanism, &c.

Class III Oxigen.
—— IV Aoxurets.

1 Simple combustibles,
2 Metals,
3 Metalloids.

Class V Oxurets.

1 Compound combustibles,
a Sulphur.
b Phosphorus.
c Azote?
d Hidrogen?
2 Acids,
3 Oxides,
4 Alkalis,
5 Earths,
6 Water.

Class VI Mineral,
—— VII Vegetable, and
—— VIII Animal compounds.

Study of a science to begin

Precise knowledge of the recondite phenomena of any science can only be attained by him, who has previously acquired

acquired its principles; the study of chemistry is therefore properly begun by an attentive consideration of those principles, by which its operations are produced and explained; and consequently that arrangement would appear to be the most proper, in which these are made to occupy the first place.

with its principles.

But I proceed to speak of the three classes, which are peculiar to the arrangement proposed. The first of these contains one substance only, namely oxigen. My reason for devoting to this matter one whole class is founded on its very extensive importance, and more especially on the relation which in this plan it bears to the two classes that follow. I believe there is no substance in nature, which is not capable of entering into combination with oxigen, except those substances which have already suffered a combination of this kind: such is the universality, and consequently such the high importance, of this chemical agent.

Classes peculiar to this arrangement.

Importance of oxigen,

This circumstance, added to the view of the relation of its combinations to each other, which I have partly given in my former communication, will, I trust, justify the plan, which I have adopted.

The fourth class comprehends those substances, which *do not* contain oxigen; the fifth comprises all substances, which *do* contain this matter. The articles of the fourth class do all combine with oxigen, and (as has been formerly stated) generally with each other: some of the individuals of the fifth class unite with a still farther quantity of oxigen, and, as in the last case, generally with each other. But no individual of the fourth class will enter into combination with any substance which is included under the fifth.

Substances in the 4th class do not combine with those in the 5th,

It has been said, that all the substances of the fourth class will combine with oxigen; they may therefore all be converted into articles of the fifth class; and hence it becomes possible, that any two substances whatever may, through the medium of oxigen, be made to combine.

except through the medium of oxigen.

I have alleged, that the substances of the fifth class will generally combine with each other; this however is not universally the case; for when this combination does take place, certain states, or degrees of oxidation, in the respective articles, are required. Many acids dissolve oxides in

Substances in the 5th class combine generally, but not universally,

requiring a certain

peculiar state of oxidation,

certain states of oxidation only. Thus if sulphate of iron be exposed in solution to the action of the atmosphere, the iron become farther oxidated, insoluble in the acid, and precipitates. Hence arises an extremely curious fact, namely, that a metallic oxide may precipitate a different oxide of the same metal. And on the other hand it sometimes happens, that an oxide will only combine with an acid when this latter contains a certain proportion of oxigen: this occurs in the mixture of the oximuriatic acid with the alkalis: a muriate and hyperoximuriate of the alkali are formed.

or disposing affinity.

Or if a combination of this kind should take place, an energetic disposing affinity is exerted, so that oxigen is attracted, and the appropriate states of oxidation come to be fulfilled; as is exemplified in the sulphurets, sulphites, phosphurets, &c.

New names.

With regard to the names, which I have suggested to express the articles of the fourth and fifth classes; I am by no means convinced of the necessity for their adoption, and less still am I satisfied with the denominations, which I have ventured to offer, especially with that of aoxuret; or, as it might be rendered, anoxuret. The term oxuret was chosen from its conformity with the general plan of chemical nomenclature, as in the examples, sulphuret, phosphuret, hidroguret, &c.; the privative particle *a* is prefixed to this term, to denote the substances of the fourth class.

Arrangement of substances as elementary or compund liable to perpetual change.

The plan, which arranges chemical substances according as they are elementary, or compound, is, from the daily advancement of chemical science, perpetually subject to change; for that substance, which we consider to day as simple, may very shortly be discovered to be compounded. The classification of substances, founded on relations in their chemical properties, since it is not liable to change from discoveries in the analysis of bodies, will be more permanent.

Advantages of the author's plan.

I imagine, that the arrangement, which I have suggested, will be allowed to possess, independantly of the advantages of the latter plan, certain others, which are peculiar to itself, and by no means unimportant. The assistance which it affords to the memory, and the extensive knowledge it

conveys

conveys of substances which admit of, or resist, mutual combination, are the advantages to which I allude.

I am, Sir,

Yours obediently,

MARSHALL HALL.

University of Edinburgh,
Dec. 13, 1810.

IX.

Remarks on Military Rockets; by Mr. HUME. *In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

THE description of the military rockets, detailed in the last number of your excellent Journal, affords me another opportunity of recognizing the schemes and proposals, which, above seven years ago, I offered to the Board of Ordnance. On this subject I have more to say than would be proper to intrude upon your pages, which ought not to be overcharged with controversial matter; but as this question is of considerable import to myself, for reasons I will not here explain, I trust to your indulgence to admit this letter into the Philosophical Journal.

Improvements offered to the Board of Ordnance by the author,

In your correspondent's communication, there are some monopolizing assumptions, concerning these instruments, against which I must enter a decided protest; particularly as I feel my character as well as name implicated in the question in all its bearings, whether it concern the rockets, or, as they were originally named, the pyrotechnic *arrows*.

and applicable to missile instruments in general.

The first of these sweeping conclusions is the following. Speaking of rockets generally, the author informs us "of their not having been used until very recently as implements of warfare."

Rockets used in the wars of India.

Now, to set this assertion at rest for ever, we have only to refer to the late wars in India, under Marquis Cornwallis;

lis; and, I should think, there were few officers on that service, who did not witness the employment of rockets as "implements of warfare." But this is not all, for in Major Dirom's "Narrative of the Campaign in India, 1792," we read of "a heavy fire of cannon, musquetry, "and *rockets*, opened from the works." The author adds, indeed, that "the fire from the garrison was *luckily* ill di- "rected." In other parts of the same volume, we have— "The enemy's line, on being approached, opened upon "them (our troops) with grape, musquetry, and rockets." —"The Sultan—sent a party of his people with rockets, to "disturb the camps during the night." In almost every chapter we may observe the military rockets are named, as well as rocket-boys, or those people, who had the chief management of this department. We are frequently reminded by the Major of the great clumsiness of their weapons, the bad discipline, the ill directed fire, and other analogous instances of the rude state of these natives, which show how far they are behind us in military science; we must, notwithstanding all this, acknowledge, that in the *use* of rockets as "implements of warfare" they have a prior claim to any man in this country.

Striking instance of this.

But I do not rest upon this single authority, although this is dated full nineteen years ago; for, in the 3d vol. of the "Asiatic Researches," there is the following paragraph, in an account of the battle of Paniput, on the 7th of January, 1761—"As the Rohillas had a great number of "rockets, they fired vollies of *two thousand at a time*, "which not only terrified the horses by their dreadful noise, "but did *so much execution also*, that the enemy (the Mah- "rattas) could not advance to the charge."

Hence, we must observe, the rockets were not appropriated to purposes of mere amusement and show, but positively as *implements of warfare, more* than *fifty years back* from the present day; for we ought not to suppose, that these instruments were all invented and constructed on the very day of the battle of Paniput.

Having thus got rid of one object, I shall now take leave to resist another; which, as it hinges upon the last inference, certainly stands upon no better foundation.

In

In a *note*, that it should not be entirely lost, we are informed that "The *invention*" of the military rocket is "exclusively due to one gentleman only." The invention of military rockets therefore

After what has been advanced on Major Dirom's authority, and the cases quoted from the "Asiatic Researches," it would be superfluous to combat this grasping assertion. There is a most obvious distinction between an inventor and an *improver*; for improvement depends upon *others*, as well as on the genius and abilities of him who undertakes the management of the article *to be improved*. No man would say, that Mr. James Watt of Glasgow *invented* the steam engine, and leave the Marquis of Worcester's "Century of Inventions," the names of Savery, Newcomen, Crawley, and others out of the question. Nor, at the same moment, would any one withold from the worthy Mr. Watt all the admiration and gratitude so justly due to this incomparable mechanic, this wonderful, this towering genius, whose name in that particular branch can never be forgotten. Upon this part of my subject I, therefore, shall offer the following as the most legitimate inference; that there is no such person now living, as the *inventor of military rockets*. not ascribable to any one now living.

The outlines of my claim to "*certain identities*", on which so much of the construction, perfection, and *efficacy* of modern rockets depend, are already before the public*; and I have never retracted a single syllable of that appeal. The *head* or *carcase* of the pyrotechnic-*arrow*, however my original design may have been tortured and garbled, was proposed by myself; and it is of little consequence to me, which end of the instrument is preferred to begin the combustion, as long as the effect eventually succeeds. Claims of the author.

That many very important improvements have succeeded since Oct. 24, 1803, I am perfectly disposed to allow; I am not inclined to arrogate to myself the title of an *inventor* of military rockets, nor can I allow it, as I have said, to any other person whatever. Circumstantial evidence is often of the utmost value, and, when complete, is generally amply conclusive—in my case this species of proof is not His suggestions improved upon.

* Gentleman's Magazine, Feb 1803; and Times, Nov. 14, 1809.

A short statement of his case.

deficient. There were no *iron* rockets in use when I had the honour of being examined by Generals Congreve, Lloyd, Drummond (since dead, I believe,) and others, who formed that Committee of Colonels and Field-officers of the Royal Artillery. As the sketches and drawings, which I took with me to assist in description, were some years in the Royal Arsenal, it is no unreasonable question to ask, why my papers were not instantly sealed up, and, if not returned to me, carefully preserved from the inspection of those, who had no *official* concern with them. That these papers were *not* so preserved, there is no difficulty in proving; and I am compelled to say in the mildest terms, that the whole transaction bears the appearance of gross partiality and injustice against myself, as an humble individual, who stipulated for neither favour nor reward. I beg, once for all, to declare, that, of the gentlemen who composed the committee, speaking of them *individually*, I have no certain reason to complain; at the same time, I must consider the whole question open on all points for discussion, if it should ever be necessary.

I am perfectly aware of a "Reply*" to one of my letters; to this however I shall never make any *specific* rejoinder, till the name of its author is acknowledged by the editor of that newspaper; when I shall most willingly attend to all that this reply contains, as well as to many important circumstances, which for plain reasons its author has concealed.

Lest I be charged with asserting any of the above particulars, I shall close this subject by transcribing the last letter I received from the Board of Ordnance; and this is given to you rather as a voucher, that I have had some *connexion* with that department, than as a proof of the validity of my claim. This letter was in answer to my last request, "to have the papers and sketches returned with an official signature annexed." I should observe, however, that, when I first applied for them, the answer contained these words, on which I shall now make no comment.—"I am to observe, that I cannot discover those papers were ever received in my office". The following is a copy of the last letter, to which I have above alluded.

* Times, October, 16.

"SIR,

Office of Ordnance,
Oct. 22, 1806.

"SIR,

"In compliance with the request, contained in your letter of the 14th instant, I herewith transmit the Sketches and Descriptions of your projectiles, which were submitted to a Committee of Colonels and Field-officers of Artillery, in October, 1803."

(Signed) R. H. CREW.

Trusting I shall not be teased with any more *anonymous* observations on this or any other attempt to vindicate my claim, I shall, for the present at least, quit this subject.

I remain, Sir,
Your obliged and obedient servant.
JOS. HUME.

Long Acre,
December 17, 1810.

X.

Remarks on the Nature of the new Metals, Potassium and Sodium, in Answer to Mr. DALTON. *In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

THE great ingenuity displayed in Mr. Dalton's new System of Chemistry, of which the second part has just appeared, must strike every one who reads it; and should his ideas prove hereafter to be really consistent with fact, the science will indeed have attained a "happy simplicity". It is not my intention to enter into a discussion of the general merits of Mr. Dalton's Theory, but I wish to call your attention, and, through the medium of your valuable Journal, that of the chemical world at large, to that part of the work alluded to, which treats of the nature of the bodies obtained from the fixed alkalis.

Mr. Dalton's theory of chemistry.

His reasons for considering potassium as a compound.

Mr. Dalton prefers the idea of potassium being a compound, rather than a simple substance, because, when potash, which has been previously fused (in which case, he says, it consists of 1 atom of water and 1 of potash) is submitted to the action of the Galvanic battery, it gives out no hidrogen: the effect of the electricity is to evolve oxigen at the positive pole, and this arises from the water of the hydrate—but the hidrogen (derived of course from the same source) draws with it an atom of potash to the negative pole, and they together constitute the compound atom potassium, consisting of potash 42 + hidrogen 1 = 43, and the residual potash is unaltered. In the French method, by the gun-barrel experiment, a quantity of hidrogen is evolved, and the results are, potassium, oxidated iron, and part of the potash employed. In this case, Mr. Dalton supposes (allowing however that the constitution of potassium here is not so obvious as when produced by electricity) that the hydrate is decomposed partly into potash and water, and partly into potassium and oxigen.

Remarks on the first.

In regard to the first conclusion, that no hidrogen is evolved, when *fused* potash is made the subject of experiment with the Voltaic battery, where is the *certainty* of the fact? At the high temperature requisite for the fusion of the potash, the potassium burns as soon as it is produced, and why not the hidrogen too? Atmospheric air is present, and we might almost as well infer, that no potassium is formed, as no hidrogen; if the proof is to rest on our inability, under these circumstances, of preserving it. We may, to be sure, catch, though with difficulty, a momentary glimpse of the one, which the nature of the other forbids. Could the decomposition of potash in fusion* be conducted in close vessels, so as to *collect* the gaseous products (and perhaps it might be done), the probability is, that *hidrogen would* be collected at the negative pole; and if so, all question as to the compound nature of potassium (at least as to its being

* The author here appears to have fallen into a mistake. Mr. Dalton is not speaking of the decomposition of potash while in a state of fusion; but of potash that has been previously fused, for the purpose of expelling from it water. C.

formed

formed by the union of potash and hidrogen) would be done away with at once.

With respect to Mr. Dalton's second idea, that the hidrate of potash in the gunbarrel experiment seems to be decomposed partly into potash and *water*, and partly into pottassium and oxigen: what is the power or affinity, which produces this mixed decomposition? Mr. Dalton has told us, p. 470 and 471, that at a red heat potash remains in tranquil fusion—"but if the heat be increased, white "fumes begin to arise copiously. The *alkali and water* "*both evaporate* in this case—therefore the process cannot "be used to expel the last portion of water from the al-"kali."—It is not therefore according to himself the *temperature* of the "intensely heated iron tube," nor its contents, that separate the *water* from the potash. We are speaking of the water of course in its undecompounded state,—Mr. Dalton's words are, (as stated above) "into potash and water". Remarks on his second argument.

Mr. Dalton calls the potash used by the French Chemists in the foregoing method of decomposition, the *first hydrate*, meaning, I suppose, that which consists of 1 particle of water + 1 of potash, or, in *plain English*, potash containing 16 per cent of water, which is exactly the potash obtained by heating any other hydrate, containing more water, to redness, and which cannot, according to himself, be farther deprived of its water by heat. What then, I ask again, is the power, that produces this separation of *water* and potash? Iron heated to whiteness will *decompose* water, we know very well, but not that it will separate it, *undecomposed*, from potash. But how does Mr. Davy explain the fact? According to him, the affinity of iron for oxigen, and of iron for potassium, renders it capable of taking the oxigen both from the potash, and the water it contains; and in this it is assisted by the affinity of hidrogen for potassium, which at a high temperature is capable of dissolving it in large quantities, but deposits it again on cooling. This, in few words, is Mr. Davy's explanation of the decomposition of potash by the gunbarrel. The reader may find it more amply detailed in a note of that Gentleman to his Bakerian Lecture, in the Philosophical Transactions for 1809, Mr. Davy's explanation of the 2d case.

1809, p. 57*. Perhaps after having carefully perused that note, in which the following sentence occurs.—"Sodium appears to be almost *insoluble in hidrogen*, and "this seems to be one reason, why it *cannot be obtained*, "except in very minute quantities, in the experiment with "the gunbarrel."—The reader may be rather surprised on turning to p. 503 of Mr. Dalton's new system, to find the following.—"Though Mr. Davy's original method of obtaining sodium by Voltaic electricity is the most instructive, as to the nature of the new product, yet that of Gay-Lussac and Thenard is the most *convenient*, when a "*quantity* of the article is required. *That is, to pass the "vapour of redhot hydrate of soda over iron turnings, in a "gunbarrel heated to whiteness.*"—Mr. Dalton thus recommends a process for procuring sodium by *wholesale*, which Mr. Davy informs us will afford it only in very minute quantities. Which of the two is to be taken as authority in this case?

Another argument of Mr. Dalton from the lightness of potassium,

To follow Mr. Dalton a little farther in his arguments for the compound nature of potassium, he says, "The "levity of it, combined with its volatility at a low red "heat, agrees with the notion of its being potash and "hidrogen, or potassetted hidrogen, resembling the other "known compounds, of sulphur, phosphorus, charcoal, "arsenic, &c. combined with hidrogen." As to its low specific gravity being a proof in favour of Mr. Dalton's idea, it is answered by Mr. Davy in his paper "on the decomposition and composition of the fixed alkalis," p. 31 of the Philosophical Transactions for 1808†, in which (having stated that it was the opinion of most of the philosophical gentlemen, whom he had consulted, that the new substances should be called metals) he observes, that their *levity* is not sufficient reason for making them a new class, for there are remarkable differences in their respective specific gravities among the metals, as for instance, between platina and tellurium, the former being nearly four times heavier than the latter,—"and in the philosophical division "of the classes of bodies, the analogy between the greater

already answered by Mr. Davy.

* Journal, vol. XXIII, p. 256. † Ibid, vol XX, p. 321.

"number

"number of properties must always be the foundation of "arrangement." Now I think Mr. Dalton will not deny this position; I would therefore ask him, whether the number of analogous properties is greatest between potassium and sodium, and the gaseous compounds of hidrogen, which he has enumerated, or between the *new* and the *common* metals. If the *new* metals be hydrurets, it follows, I think indisputably, that the common metals must be hydrurets; also and for that matter, the reasoning derived from their specific gravities makes as much against as for the argument, or rather it is decidedly adverse to it.

Mr. Dalton's remarks on the combustion of potassium in muriatic acid.

I have already exceeded the limits, to which I intended to have confined my remarks on this part of Mr. Dalton's new system; I shall therefore but briefly notice his observations on the combustion of potassium in muriatic acid. It is very singular, as he says, that the French chemists, and Mr. Davy, should have adopted the same explanation to account for the hidrogen produced, their views of the nature of potassium being so different. That, at the time he made the experiment, Mr. Davy should have concluded, that water contained in the gas was the source of the hidrogen, was perfectly natural; but, if Mr. Dalton be correct, water cannot exist, either in this gas, or in fluoric acid gas, (vide new System, page 282) and consequently Mr. Davy's conclusion must have been wrong; but Mr. Dalton's, and that of Gay-Lussac and Thenard, that it came from the potassium, are not more correct, for the late admirable "Researches on the oximuriatic acid" &c., as detailed in Mr. Davy's last paper, Phil. Trans. 1810, page 231*, prove, that the true origin of the hidrogen was from the muriatic acid itself, independent of water. This acid being composed of the peculiar simple body, oximuriatic acid (I must call it by its old improper name, for want of a better), with a base of hidrogen; by the combustion of potassium in which a compound of oximuriatic acid and potassium is formed, and the base evolved.

Mr. Davy's explanation of it.

The experiments prove muriatic acid

Mr. Dalton, in his appendix, mentions Mr. Davy's paper just alluded to, but still retains his opinion, that the hidro-

* Journal, vol. XXVII, p 321.

gen

to be a compound of hidrogen and an acidifying substance.

gen results from the decomposition of the potassium. I cannot but think the facts prove absolutely the contrary, and that they are *supported* by the experiment of the combustion of equal parts of oximuriatic acid and hidrogen gasses, which gave muriatic acid for the result; the accuracy of which I can have no doubt of, although Mr. Dalton seems to hint, that implicit reliance on that head is not to be placed on it. Mr. Davy says it was attended with a condensation of $\frac{1}{10}$ or $\frac{1}{12}$ of the volume, and the deposition of a slight vapour ensued, which was always in smallest quantity when the gasses were driest. Mr. Dalton says, that he should have expected the condensation to have amounted to $\frac{1}{4}$ or $\frac{1}{5}$ on the common hypothesis; by which I understand, that he still considers oximuriatic gas as a compound of muriatic acid and oxigen. Now viewing it in this light, oximuriatic acid consists, according to Chenevix, of 77·5 muriatic acid + 22·5 oxigen by weight. In the combustion therefore of 100 measures of this gas, with an equal quantity of hidrogen, the oxigen (reckoning its specific gravity as 1·125, atmospheric air being 1·000) would take 60·48 measures of hidrogen, and a condensation of 90·72 would ensue, nearly half the whole quantity, and vapour in considerable abundance would be the consequence; but the slight portion mentioned by Mr. Davy resulted from another cause, as proved by its quantity diminishing, in proportion to the dryness and purity of the gasses employed. This immense difference between the condensation that does actually take place, and what ought to occur, according to the common hypothesis, is, I think, an incontrovertible argument in favour of Mr. Davy's conclusion, that muriatic acid is composed of a base of hidrogen united to a peculiar body, a substance *sui generis*, into which not a particle of oxigen enters, however similar the phenomena attending its union with many bodies may be to those produced by the combustion of inflammable substances in oxigen gas; since, as Mr. Davy justly remarks, the evolution of light and heat are only to be regarded as evidences of intense combination. With respect to the oxigen of the hyperoximuriate of potash, as it is called, the idea that it is furnished by the alkali seems to me to be the

the true one, which in this case is in the state of peroxide; but, in its common form (potash) it is only a protoxide.

"Potassium burns in fluoric acid gas, and the result is "fluate of potash and some hidrogen. The theory of this is not "obvious." According to Mr. Dalton's views it is not; but is it too bold a conjecture to imagine, that something similar to what takes place, when potassium is burnt in muriatic acid gas, occurs here also? and that hidrogen is probably the base of fluoric acid gas, united perhaps to some, as yet unknown body, of the same class as oximuriatic gas? Fluoric acid perhaps a similar compound.

As the similarity between the properties of potassium and sodium is so great, I omit saying any thing on the latter, more than the remark already made, respecting the difference in the results with potash and soda in the gun-barrel experiment. The arguments for the undecompounded nature of potassium are equally applicable to that of sodium. The same arguments apply to sodium as to potassium.

From a candid review of all the facts, I think it must be allowed, that Mr. Dalton does not make out his case; and that potassium and sodium consequently stand in the class, where their illustrious discoverer has placed them. It will be observed, that almost all the arguments, which I have brought forward in opposition to Mr. Dalton's idea of the compound nature of the new metals, are derived from Mr. Davy's papers: and it is not small praise, that he has sent his labours so perfect into the world (new and difficult as they have been), that his own conclusions, founded on and supported by his own experiments, form the ablest and most perfect defence against all the attempts, that have been made to overthrow them. Mr Dalton's opinions respecting them therefore not proved.

JUSTUS.

XI.

Observations on the Diminution of Dilatability of Spirit of Wine Thermometers; by Honore Flaugergues*.

HALLEY and Muschembroeck have asserted, that spirit of wine employed for thermometers in course of time Spirit thermometers dimi-

* Journal de Physique, vol. LXVI, p. 295.

loses

Spirit in dilatability.

loses part of its dilatability. Nollet and Brisson maintain the contrary, on the experience, as they say, of more than thirty years. The two following facts however confirm the opinions of Halley and Muschembroeck.

This shown in one made by Nollet,

I have a spirit of wine thermometer, made in 1734 by Mr. Nollet, on the principles and under the inspection of Reaumur. From fear, no doubt, that the wire marking the freezing point might be displaced, Mr. Nollet wrote with his own hand on a paper pasted on the inside of the lid of the case of the thermometer the following note: "The upper extremity of the stem of the thermometer "ought to be at 94°:" and at this point he has made a groove. Hence it was easy for me to place it exactly in the same situation, as when it came out of the hands of that able philosopher. When I was thoroughly certain of this, I placed the thermometer in a vessel full of pounded ice, just melting; and in this state I kept it three days, adding fresh ice, when the former was half melted, and draining off the water. The liquor in the thermometer settled a degree and a quarter below nought, and remained stationary at this point during the whole time it remained in the ice. This experiment was made on the 7th of March, 1807, and the two following days.

and another by Romieu.

I have another spirit thermometer made before 1758, according to Reaumur's principles, by Mr. Romieu, a learned natural philosopher of the Royal Society of Sciences at Montpellier, whose accuracy is generally known. This thermometer is enclosed and fixed in a large glass tube hermetically sealed, so that it cannot have been deranged. I placed this at the same time in a vessel of melting pounded ice, with which it was completely surrounded; and during the three days that this experiment continued, the spirit of wine continued stationary at two degrees and a quarter below nought.

Intended to be tried again at a future period.

I preserve these thermometers, though grown very defective, in order to try whether their dilatability will diminish still further. In the mean time these observations appear to me sufficient, to induce us to reject the use of spirit of wine thermometers; and keep to those of mercury, which do not appear liable to the same inconvenience.

REMARKS.

REMARKS.

In a subsequent number of the Journal de Physique Mr. Cotte observes, it is somewhat surprising, that the newest thermometer should have lost most: and he reminds Mr. Flaugergues, that in 1734 de Réaumur and Nollet fixed the freezing point by water just beginning to freeze, which he adds, according to Mr. De Luc's experiments, differs 0·8° from that of melting ice. By an oversight however he deducts these 0·8° from 1·25°, and thus reduces the loss of the thermometer to half a degree at most in 73 years. Correction of the preceding statement,

Mr. Flaugergues in reply admits the adjustment of the freezing point; and very properly subjoins, that, as he had placed it so much too low, the thermometer in reality did not rise so high, as when it was made, by 1·25°.+ 0·8°, or 2·05°, which amounts to nearly the same as the loss of Romieu's, the freezing point of which was no doubt that of melting ice. This therefore only tends to strengthen his observations; and he remarks, that two thermometers made by different persons, at very different periods, can hardly be expected to have contained spirit of precisely the same strength; which accounts for one having lost in dilatability rather more than the other in about three fourths of the time. which tends farther to confirm it.

It is to be regretted, that Mr. Flaugergues did not examine the dilatability of the spirit in higher temperatures also.

C.

XII.

Analysis of the Amphibole of Cape de Gattes, in the Kingdom of Grenada: by Mr. LAUGIER*.

THIS amphibole is found among the volcanic products at Cape de Gattes. Its crystals have a black hue, and are Hornblende among the volcanic products

* Annales de Chimie, vol. LXVI, p. 325. Abridged from the Annales du Muséum d'Hist. Nat. by Mr. Bouillon-Lagrange.

formed

of Cape de Gattes described.

formed of a multitude of laminæ, which give them a chatoyant appearance. Their fracture is transverse, and uneven. Spec. grav. 3·25. They scratch glass; and strike fire with steel, but not easily. Their primitive form and integrant particle are an oblique prism terminated by rhombs. Before the blowpipe they melt into a black glass. It breaks pretty readily; but is not easy to reduce to powder, more on account of the flexibility of its laminæ than its hardness. The course powder is of a dull green, the very fine powder of a greenish gray.

When exposed to a red heat it loses about 2 per cent, and retains on cooling a reddish hue from the oxide of iron contained in it.

Comparison between it and strahlstein.

The principal object of Mr. Laugier in analysing this substance was to compare the nature and proportions of the component parts of amphibole and actinote, between which crystallography finds a perfect resemblance. Though the results do not exhibit a resemblance so complete; Mr. Laugier does not think they differ so sensibly, but some resemblance may be found between the two stones. If the proportions of the substance that compose them be not exactly the same, at least we find in them the same principles*; and it appears no less established, that the differences found in them are not any of those that influence the form of the crystals. The absence of chrome and of an atom of potash in the amphibole; the presence of a larger portion of alumine, and a double quantity of iron in the same stone; and lastly, some other differences in the proportions of the remaining principles; do not essentially produce any very remarkable change in the crystallization, according to the observations of the most celebrated mineralogists†. It may not be useless to observe, that the difference between the proportions of the absolute quantities

* Not wholly. C.

† That is to say, whether a stone contain 11 per cent of oxide of iron, or above twice as much; about 10 per cent of magnesia, or almost 20; near 8 per cent of alumine, or not 1 per cent; 3 per cent of chrome, or not a single atom: the form of its crystals may be precisely the same, and they may exhibit no difference to their most expert dissectors. C.

of

of the principles of these two stones is much less considerable, than that which exists between these quantities themselves, when they are compared. Thus the result of this comparative analysis appears to bring actinote and amphibole so near together, that it seems necessary to include them in one species, as modern mineralogy has already indicated.

The following are the results of Mr. Langier's analysis of amphibole, compared with those of actinote.

	Amphibole.	Actinote.	Component parts of the two.
Silex	42	50	
Oxide of iron	22·69	11	
Magnesia	10·90	19·25	
Lime	9·80	9·75	
Alumine	7·69	0·75	
Oxide of manganese	1·15	0·50	
Chrome		3	
Potash		0·50	
Water and loss	5·75	5·25	
	100 *	100	

SCIENTIFIC NEWS.

Royal Society of Edinburgh.

ON Monday, the 5th of November, the Royal Society of Edinburgh met for the first time in their new apartments in George street, when Dr. Thomas Thomson read two papers, giving an account of the analyses of two new minerals from Greenland. To one of these he has given the name of *allanite*, and to the other that of *sodalite*. In the first he discovered a considerable portion of cerium; and in one analysis he detected a quantity of a metallic oxide perfectly new in its

Two new minerals,

one containing a new metallic oxide,

* These numbers are here as in the original; but the sum total would amount only to 99 98, so that there is an omission of 0 02 somewhere. C.

its properties, for which he proposed the name of junonium.

the other 0·23 of soda, and 0·3 of muriatic acid.

The other mineral according to his investigation affords 23 per cent of soda, and 3 of muriatic acid. By an analysis of Mr. Ekeberg, the same constituents were yielded in the proportions of 25 per cent, and 6 per cent.

Waterspout.

At the next meeting, on the 19th, a short communication was read respecting a singular waterspout, observed at Ramsgate.

Properties of the lever.

On the 3d of December a paper by Dr. Brewster was read, being a new demonstration of the fundamental properties of the lever. Also a communication by sir George

Mineralogy of Iceland.

Mackenzie, bart., relative to the hot springs of Iceland, when sir George exhibited some beautiful drawings, and part of a series of magnificent specimens, from that country, which he proposes to deposit in the cabinet of the society; and at the last meeting, on the 17th, sir George began a description of the minerals of Iceland, when he exhibited specimens from the district called the Guldbringe Syssel.

New Agricultural and Commercial Magazine.

Mr. Clennell, of Homerton, whose institution of a Literary and Philosophical Society at Hackney we noticed a few months ago, will commence with the new year the publication of a "New Agricultural and Commercial Magazine; or general Depository of Art, Manufactures, and Commerce." It is obvious, that the subjects included under these heads form the basis of the comforts, enjoyments, and interests of individuals, and thence of society at large; and, from the names of those who have already engaged to contribute to the undertaking, much useful information and free and candid discussion may be expected in it. The term local has frequently operated as a bugbear, to prevent the communication of much valuable knowledge: but all our general information must originate from particular facts; and every fact is from its nature, in one sense of the word, local; while no one can be so strictly confined in its bearings and relations to this town, or that village, as to be incapable of furnishing useful hints or cautions to numberless others, if not to every one. Of this the conductor is fully aware, and

and accordingly solicits accurate and faithful information on all the subjects connected with his plan from every quarter.

Translation of Werner on Veins.

Werner's Theory of the Formation of Veins, with its Application to the Art of Working Mines, has been translated from the German, by Dr. Charles Anderson, of Edinburgh, member of the Wernerian Society, &c. It is accompanied with notes by the translator, some of which relate to mine districts in our island not noticed by Werner. It is unnecessary to say more of a work published about twenty years ago, than that it exhibits the results of nearly thirty years investigation by a man, whose experience and sagacity as a mineralogist have long been acknowledged.

Treatises on diseases of the eye.

Dr. Joseph Reade, of Cork, has just issuing from the press some Critical and Practical Observations on the Diseases of the inner Corner of the human Eye, comprising the epiphora, tumour of the lachrymal sac, and fistula lachrymalis, with a new arrangement and method of cure.

To Correspondents.

The communications from Mrs. Ibbetson, Mr. Dalton, Mr. Murray, and Mr. Bowditch, were too late for the present month, but will be inserted in the next number.

Meteorological

METEOROLOGICAL JOURNAL,

For DECEMBER, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

NOV. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
27	44°	45°	47°	38°	29·19		Cloudy	Cloudy*
28	42	45	47	35·5	·04	·095	Ditto	Rain
29	40	40·5	43	34	·02	·015	Rain	Fair
30	37	38·5	42·5	32	·18	·240	Fair	Ditto
DEC.								
1	35	37·5	39	30·5	·45		Ditto	Cloudy*
2	33	34	37	28	·92		Ditto*	Fair*
3	33·5	41·5	42	39	·95		Rain	Cloudy
4	42	47·5	48	42	·91	·090	Fair	Rain
5	46·5	48	49·5	45	·92	·025	Cloudy	Ditto
6	48	47	50	41·5	·60	·065	Rain	Fair
7	44	41	45·5	34	·33	·070	Fair	Ditto
8	37	36	41	30	·52		Rain	Ditto
9	32	34·5	36	30·5	·86	·015	Fair*	Ditto*
10	37	36·5	41	34	·55	·345	Rain	Cloudy
11	35	35	37	29·5	·60	·250	Fair	Fair
12	35·5	44	49	40	·73		Rain	Ditto
13	47*	50·5	51	46	·82	·120	Cloudy	Rain†
14	50·5‡	45	50·5	38	·63	·210	Rain	Fair
15	42·5	44·5	45·5	37	·86	·170	Fair	Ditto
16	41	39	42	38	30·22		Fog	Ditto*
17	41	47	48	44	·23		Cloudy	Cloudy*
18	48	41·5	48·5	38	29·63	·030	Rain	Fair
19	41	42	42	34	·47	·330	Fair	Ditto*
20	37	45	48	37	·85		Rain	Ditto
21	44	43	48	38	·33	·110	Ditto	Fair
22	42	50	52	46	·62	·040	Ditto	Rain
23	48	45	53	42	·40	·260	Ditto	Fair
24	44	42	50	38.5	·31	·350	Ditto	Ditto
25	48	46	49·5	41	·31	·200	Ditto§	Cloudy
26	45	45·5	51	41	·58	·285	Fair	Ditto
						3·315 Inch.		

* Rain in the night. * Intervening fogs. † Stormy night. ‡ Maximum of Heat at 9 A. M. § Stormy morning, hail at half past 4 P. M. lightning in the evening, and stormy night.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1811.

ARTICLE I.

Inquiries concerning the Signification of the word Particle, as used by modern Chemical Writers, as well as concerning some other Terms and Phrases. By Mr. JOHN DALTON. *In a Letter from the Author.*

To Mr. NICHOLSON.

SIR.

IN perusing the modern books, which are edited under the titles of Elements or Systems of Chemistry, I have been greatly perplexed in endeavouring to understand the meaning, which the authors attach to certain words and phrases, which are of perpetual occurrence, particularly when treating of *affinity*, but which the authors no where define. I allude more especially to the word *particle*. Meaning of chemical terms undefined.

With respect to the article *affinity* itself it is somewhat remarkable, that different authors should view it in such different lights. Lavoisier in his Elements of Chemistry does not say a word on the subject, except a few observations in his preface tending to show the difficulties of it, and its unfitness to be presented to a novice in chemistry. In Affinity.

speaking of the number and nature of elements, he says, "I shall therefore only add on this subject, that, if by the term *elements* we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable we know nothing at all about them." Chaptal employs about 20 pages on affinity, two on the affinity of aggregation, and the rest on the affinity of composition. In the third edition of your First Principles of Chemistry, I observe 10 pages on affinity. Berthollet has written a small volume on the subject. Fourcroy, Thomson, Murray, and Henry, have all expanded their views on this subject pretty extensively. From these observations it should seem, that the article *affinity* in systems of chemistry has been of *growing* importance since the time of Lavoisier's publication. This may certainly be right; but as the subject is acknowledged to be one of the most difficult in the science, it is highly necessary, that authors should treat it with all the perspicuity of which it is capable; the terms used should be clear and well defined; metaphorical expressions should be avoided; and ambiguities should be guarded against with all possible care.

Chaptal.

Chaptal has been very sparing in the use of the term *particle* when treating of affinity (I refer to your translation, 2d ed.); he only mentions it three times, twice unqualified, and once along with the epithet *elementary*; he seems to use *integrant part*, where others would use particle, and he defines it thus: "two drops of water, which unite together into one, form an aggregate, of which each drop is known by the name of an integrant part."

Integrant part.

Nicholson's First Principles.

On the subject of affinity I observe you use the term *particles* frequently, but only once in the singular number. No definition of it is ever given; but the last time it occurs is in a very judicious concluding remark, which I do not remember to have seen in any subsequent writer; namely, "it seems reasonable to infer, that two compounded particles coming together by attraction, undisturbed by any other cause, should dispose themselves so as to apply such sides of each together, as are occupied by principles the most attractive of each other." In another part of the chapter you observe, that "the minutest parts, into which

an

an aggregate can be *imagined to be* divided without decomposition, are called integrant parts; but the parts into which it is divided by decomposition are called component parts or principles." These definitions would be clear and intelligible to me, if the words I have put in italics were omitted*; the sense affixed to *integrant parts* is better than that of Chaptal, and is the same as the most correct modern writers apply to *integrant particles*. But, after thus defining the terms *integrant parts*, I do not find, that you have again used them throughout the chapter. Integrant and component parts.

From the high encomiums, which several modern writers pass on Berthollet's researches into the laws of chemical affinity, I was induced to hope, that in a volume written exclusively upon affinity by so able a hand, I should find the term *particle* clearly defined in the first page. I took up the English translation, and read 37 pages without once meeting with the word *particle*, or any other word of similar import. I was beginning to think, that the author intended to abolish the old doctrine of bodies consisting of extremely small parts bound together by a principle of cohesion, or affinity of aggregation, and to establish the one that bodies consist entirely of cohesion, when in the opening of the 5th article I read, "the cohesion of the moleculæ of a body is due to the reciprocal affinity of these moleculæ;" the word moleculæ did not again occur in the volume to my cursory observation. In the same page I met with the phrase *integral parts*, and afterward in the space of 150 pages the words *part* and *particle* were observed in about a dozen places. Whatever obscurity may be found in Berthollet's researches then, they do not arise from the frequent use of the term *particle*. Berthollet on chemical affinity. Moleculæ.

* I do not know how Mr. Dalton would understand the passage, if the words in italics were omitted; but they appear necessary, to express the true meaning with precision. Suppose a single atom of sulphuric acid combines with a single atom of mineral alkali to form a particle of sulphate of soda, and a crystal to be formed by the aggregation of such particles: we cannot actually divide it into these component parts, though we can imagine it so divided. The word *imagined* may be here taken as synonimous with the word *conceived* of the geometers, and has no necessary relation to operative or human practicability. N.

Dr Thomson on affinity.

Dr. Thomson, in a short essay on affinity in general, uses the term *particle* frequently, and without any direct definition; but as he advances his idea of *particle* is gradually developed. Indeed he is almost the only writer, who has courage to talk of *a particle*, *two particles*, &c. *Homogeneous* particles are those of the same body; *heterogeneous* particles those of different bodies; thus "a particle of iron and a particle of lead are heterogeneous." Again, "an integrant particle of water is composed of particles of hidrogen and oxigen urged towards each other, and kept at an insensible distance, by heterogeneous affinity; and a mass of water is composed of an indefinite number of integrant particles of that fluid, urged towards each other by homogeneous affinity." He supposes the particles of matter to have figure and magnitude, and adds, "if the particles of bodies have length, breadth, and thickness, we cannot avoid conceiving them as composed of an indeterminate number of still more minute particles or atoms. Now the affinity of two integrant particles for each other must be the sum of the attractions of all the atoms in each of these particles for all the atoms in the other," &c. Here the meaning of a *particle* is very clearly pointed out. Bodies, such as iron, are constituted of *particles* connected together by homogeneous affinity; these particles are each of them constituted of a certain unknown number of subordinate particles, which last are the atoms or ultimate particles, into which the body is capable of being resolved without decomposition. This notion is clear as far as it goes (I mean not as to its truth, but as to the expression); it would seem however to require something to be said about the *cohesion* of the atoms to form a particle, namely, whether it is the same or different from the cohesion of the particles to form a mass; in other words, whether the force of cohesion, which binds together the atoms or first order of particles, is the same as that which binds the second order of particles. See Thomson's Chemistry, 3d ed. vol. iii.

Murray's Chemistry.

Mr. Murray uses the term *particles* frequently; he commences his work with the observation, that "all the phenomena of chemistry arise from the attractions and repulsions exerted between the particles of matter;" but I have not been

been so fortunate as to meet with his definition of the unqualified term *particle*. He has however given the best definition I have yet seen of *integrant particles*. "The calcareous spar, to take it as an example, may be reduced to a particle, beyond which the division cannot be carried without resolving it into its elements, lime and carbonic acid; or, at least, it may be reduced to a particle, beyond which, if its minuteness allowed us to operate upon it, it is demonstrable its figure would not change. To these last particles, the result of the mechanical analysis, Hauy gives the name of *integrant particles*, and their union constitutes the crystal." See Chemistry, 1st edit. vol. i, page 56. Again, on *integrant* and *constituent* particles, "the constituent or component parts are substances differing in their nature from each other, and from the substances they form. The integrant parts are precisely similar to each other, and to the general mass, which is composed by their union; or they are *merely* the smallest particles into which a substance can be resolved without decomposition; while decomposition is always implied in the division of a body into its constituent particles. The integrant parts are united by the force of aggregation, the constituent parts by chemical affinity," &c. All this is more than perspicuous; it is excellent; and from the familiar manner in which Mr. Murray introduces "the smallest particles, into which a substance can be resolved without decomposition," and from the precise definition of them, one would be inclined to believe the author was going to say something more about them in the sequel; but he scarcely mentions them again, while the unqualified term *particles* is perpetually introduced.

Dr. Henry adopts the common distinction of the attraction of aggregation, and that of composition, or affinity. "In simple bodies, therefore, the cohesive affinity alone is exerted; but compounds are influenced by both affinities, their *constituent* or dissimilar parts being united by chemical affinity, and their *integrant* or similar parts by the affinity of aggregation." Chemistry, 6th ed. vol. i, page 52. He does not however clearly define *integrant parts;* it is not sufficient to say, that "a lump of copper may be considered Henry's Chemistry.

as composed of an infinite number of minute particles or integrant parts, each of which has precisely the same properties as those that belong to the whole mass." For, it is obvious, such *integrant parts* may either be Dr. Thomson's *particles* (of the first order) or Mr. Murray's smallest particles into which a substance can be resolved without decomposition, which I call atoms. Dr. Henry uses the term *particles* at times in the same indefinite manner as his predecessors.

Words improperly used.

Ratio.

There are some other current expressions among chemical writers on affinity, that are either erroneous or misunderstood. Chaptal says, that "the affinity of composition is in the inverse ratio of the affinity of aggregation." Dr. Henry has adopted a similar expression, page 53. The only thing that can be intended by this expression is, that the affinity of composition is less efficacious, as the affinity of aggregation is greater; but not the affinity has precisely *half* the effect, when that of aggregation is *double*.

Berthollet on the effect of quantity on affinity,

Some new opinions and expressions were introduced by Berthollet, which want both confirmation and illustration; one of them is, that the effect of affinity is in proportion to the weight of a body multiplied by the degree of its affinity. Mr. Murray, page 85, has it: "the chemical action of any body is exerted in the ratio of its affinity and quantity." Dr. Henry observes, page 72, "to obtain a measure of the action of two bodies on a third, if their respective affinities were precisely determined, it would only be necessary to multiply the number indicating the affinity by the quantity." Berthollet's language on this subject would seem to warrant this conclusion as his; but the experiments he adduces certainly warrant no more than that the effect "is modified considerably by the mass," as Dr. Thomson expresses it. Mr. Murray however has attempted to illustrate the *truth* of the principle (Appendix, page 19), and Dr. Henry to explain its *meaning* (page 72), according to the sense in which they understand it. The latter has succeeded very well; but the former, having a much more difficult task, has, I think, still left some obscurity about it. The explanation is this: "if a particle of sulphate of barytes be on every side in contact with particles of potassia,

what

what additional force can be gained from an additional quantity of potassia, which must be without the sphere of action?" This is a very proper statement of the question. I wish the author had kept closely to it, and not substituted for it another and more complex one. Mr. M. continues: "this objection however may be obviated, by considering the manner in which quantity operates. Suppose two bodies, A and B, to be presented to each other, the particles of each within the requisite distance will combine together in that proportion, in which they will be mutually saturated. Suppose it to be the one which is in larger proportion; its mass will continue to operate, and, even if there be no successive application of it to B, from the motion attending the combination, its uncombined or unsaturated particles will take *part of the particles* of B which have entered into the combination; so that the *whole particles* of B which have been attracted by A, instead of remaining combined with part of it, in the proportion in which saturation takes place, will be diffused through the mass; and hence by an increase in this mass there will be an addition to the force of affinity." Mr. M. then adds, "no law with regard to chemical affinity is more important than that now illustrated." But the illustration is almost lost upon me, for want of knowing the precise meaning of *particle*.

and on that of the progress of saturation.

Another of Berthollet's observations is, "that the action of a substance, which tends to decompose a combination, diminishes in proportion as its saturation advances; for this substance may in such case be considered as composed of two parts, one of which is saturated, and the other free. The former may be considered as inert, and as unconnected with the latter, the quantity of which diminishes according as the saturation advances; while on the contrary the action of that which has been eliminated increases in proportion to the augmentation of its quantity, until the equilibrium of the contending forces ends the operation, and limits the effect." Researches, page 15. I apprehend it is to this that Mr. Murray alludes, page 82, when he deduces the law, "that the force of attraction is in the inverse ratio of saturation, or that the particles which enter first into combination are retained by a stronger affinity than those afterward

ward combined;" and Dr. Henry, page 73, when he says, "the force of affinity is in the inverse proportion of neutralization." This principle Dr. H. explains numerically by supposing 100 parts of potash to be divided into portions, and successively put into sulphuric acid, the first portion, he argues, will be held more forcibly than the second, &c. Now whatever errours Mr. Berthollet may have fallen into, I think he cannot be fairly charged with this; his commentators here appear to me to be responsible. Suppose, instead of potash, barytes is substituted; would these gentlemen say, Berthollet maintains the first formed sulphate of barytes would be with more difficulty decomposed than the last formed? I think no such inference can be fairly drawn from his position. If I understand Berthollet, he means, that, if a combination, as sulphate of potash, have another substance put to it, tending to decompose the combination, as barytes, the action of the barytes will be less and less powerful as the decomposition goes forward; the great quantity of potash liberated will lend its assistance to that still in combination, and both finally will prevent the small quantity of barytes remaining from effecting a complete decomposition of the sulphate of potash.

Here mistaken by his commentators.

His meaning explained.

I would not have it to be understood, that by these inquiries I wish to depreciate your publication on chemistry, or those of the ingenious gentlemen whom I have had occasion to mention. The works have their excellencies; and if they should also have their defects, it is of interest to the public and to the authors to be made acquainted with them. I hope either you, or some of your readers, will undertake to explain what you conceive chemical authors signify by *particles*; and then I think we shall be enabled soon to dispel the mists, that envelope the fundamental principles and operations of the science.

I am yours,

J. DALTON.

Manchester, Dec. 19th.

Nicholson's Philos. Journal, Vol. XXVIII. Pl. II.

Calculation of the Height of Meteors.

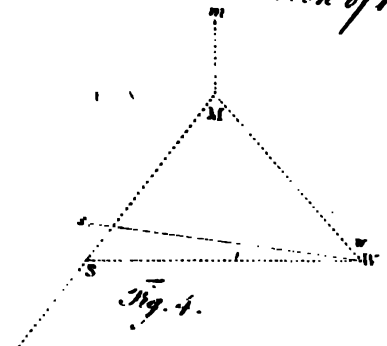


Fig. 4.

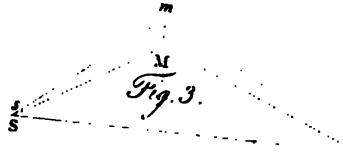


Fig. 3.

Fig. 1.

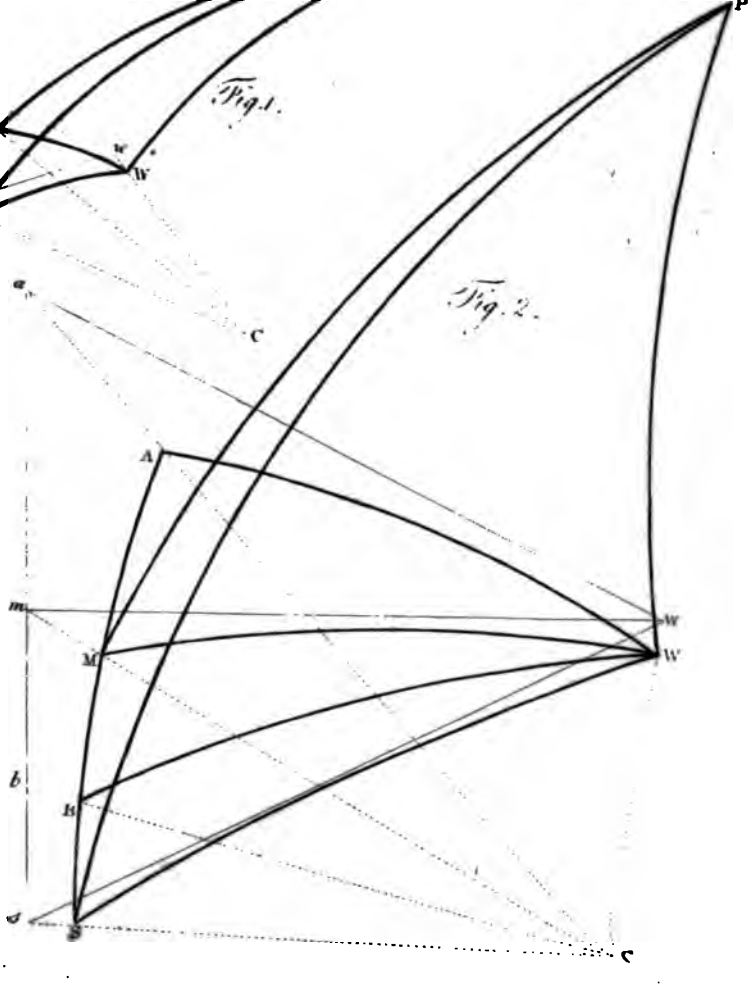
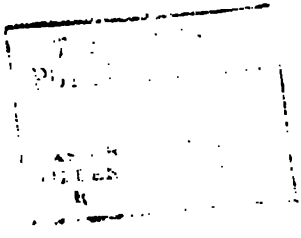


Fig. 2.



II.

An Estimate of the Height, Direction, Velocity, and Magnitude of the Meteor, that exploded over Weston, in Connecticut, December the 14th, 1807: with Methods of calculating Observations made on such Bodies. By NATHANIEL BOWDITCH, *A. M. A. A. S. and Member of the Philosophical Society held at Philadelphia.*[*]

THE extraordinary meteor, which appeared at Weston, in Connecticut, on the 14th of December, 1807, and exploded with several discharges of stones, having excited great attention throughout the United States; and being one of those phenomena, of which few exact observations are to be found in the history of physical science; I have thought, that a collection of the best observations of its appearance at different places, with the necessary deductions for determining, as accurately as possible, the height, direction, velocity, and magnitude of the body, would not be unacceptable to the Academy; since facts of this kind, beside being objects of great curiosity, may be useful in the investigation of the origin and nature of these meteors; and as the methods of making these calculations are not fully explained in any treatise of trigonometry common in this country, I have given the solutions of two of the most necessary problems, with examples calculated at full length. The second problem is not (to my knowledge) given in any treatise of spherics. The observations of the meteor, which, after many inquiries, were found to have been made with sufficient accuracy to be introduced in the present investigation, were those made at Wenham, about seven miles north-easterly of Salem, by Mrs. Gardner, a very intelligent lady, who had an opportunity of observing it with great attention; those at Weston by Judge Wheeler and Mr. Staples; and those at Rutland in Vermont by William Paige, Esq.

Meteor that exploded over Weston, in Connecticut.

Observations of it.

[*] Memoirs of the American Academy of Arts and Sciences, Vol. III, Part II, p. 213. Communicated by the author.

In

In collecting and combining these observations I have received great assistance from my friend, John Pickering, jun. Esq. A.A.S., particularly in the observations at Wenham. These observations are given after the problems, and the results from combining them in various manners are stated at the end of this memoir.

Problem 1. *Plate* III. *Fig.* 1.

Methods of determining the place of a meteor.

Suppose that in two places in given latitudes and longitudes the azimuths of a meteor were observed at the same moment of time, and its angular elevation above the horizon of one of those places. It is required to determine the situation of the meteor.

Solution.

Let C be the centre of the Earth, PWSM a portion of its surface reduced to the level of the sea supposed to be spherical, P the pole of the Earth, w, s, the places of observation, m the place of the meteor. Draw Cw, Cs, Cm, cutting the sperical surface in the points W, S, M. Then Ww, Ss, will represent the vertical heights of the places of observation, and Mm the vertical height of the meteor above the level of the sea; PWM the azimuth of the meteor observed at w, and PSM its azimuth observed at s. Then in the spherical triangle PWS are given PW, PS, the colatitudes of the places of observation, and the angle WPS equal to their difference of longitude, to find by spherics the angles PWS, PSW, and the side SW. The sum or difference of PWS, PWM, will give the angle SWM; and the sum or difference of PSM, PSW, will give the angle WSM. Then in the spheric triangle WSM will be given the angles SWM, WSM, and the side SW, to find the sides WM, SM, which are respectively equal to the angles mCw, sCm. The altitude of the meteor observed at w, added to 90°, and $\frac{1}{14}$th part of the arch WM subtracted from the sum for terrestrial refraction, will leave the correct value of the angle C$w m$: this added to mCw, and the sum subtracted from 180°, will leave the angle C$m w$. Then in the plane triangle C$w m$, will be given the angles and the side Cw, (which may in general, when Ww is small, be taken equal to

to the semidiameter of the Earth, 3982 miles*) to find wm the distance of the meteor from the observer at w, and Cm its distance from the centre of the Earth, from which subtracting CM equal to 3982 miles, there will remain the vertical altitude of the meteor above the level of the sea. In the plane triangle sCm are given Cs, Cm, and the included angle sCm, (= arch SM) to find sm the distance of the meteor from the observer at s, and the angle Csm equal to the supplement of the zenith distance of the meteor at s. The colatitude of the meteor is equal to the arch PM, and the angle SPM is equal to the difference of meridians between the meteor and the observer at s. These quantities may be easily found, by means of the spheric triangle PSM, in which PS, SM and the angle PSM are given. They may also be found in a more simple manner, and to a sufficient degree of accuracy, by the usual rules of navigation, supposing the angle PSM to be the course and SM the distance, whence may be found the difference of latitude, departure, and difference of longitude between the points S, M.

Example.

Suppose the azimuth of the meteor at Wenham PWM = 117° 35′ 54″, azimuth at Weston PSM = 3°, altitude at Wenham 5° 50′ 30″, colatitude of Wenham PW = 47° 19′ 45″, colatitude of Weston PS 48° 45′, difference of meridians WPS = 2° 36′ 45″. It is required to find the latitude and longitude of the meteor, its distance from Wenham and Weston, and its vertical height above the level of the sea. The first problem exemplified.

This corresponds to Example 10 in Table 1.

* The mile made use of in this memoir is the statute mile of 5280 feet. In the following calculations on the Weston meteor, it will be supposed that $Cw = Cs = 3982$ miles; the part ww or Ss being but a small fraction of a mile.

In

In the triangle PSW.

½PS = 24°29′30″
½PW = 23 59 52

Sum	48	2	22	Cos. AC	0·1748214		Sine AC	0·1286575	PSW	52°56′34″	Sine AC	0·0979786
Diff.	0	42	38	Cos.	9·9999666		Sine	8·0934643	PW	47 19 45	Sine	9·8664408
½WPS	1	18	22	Cotan.	11·6120673		Cot.	11·6420373	WPS	2 36 45	Sine	8·6587892
½Sum	89	7	36	Tang.	11·8168556	½diff. 86°11′2″	Tan.	9·8641891	SW	2 24 25	Sine	8·6292096
½Diff.	36	11	2			PWS 125 18 38			PSW 52 56 34			
						PWM 117 35 54		Azim.	PSM 3			
Sum	125	18	38=PWS									
Diff.	52	56	34=PSW			SWM 7 42 44			WSM 55 56 34			

In the triangle WSM.

½WSM 27 58 17
½SWM 3 51 22

Sum	31	49	39	Cos. AC	0·0707652			Sine AC	0·2778893	
Diff.	24	6	55	Cos.	9·9603401			Sine	9·6112706	
½SW	1	12	12½	Tang.	8·8223773			Tang.	8·5223773	
½Sum	1	17	34	Tang.	8·3534826	½dif. 0°55′57″		Tang.	8·2115377	
½Diff.	0	55	57							
						Cwm=95°41′50″=Alt. 5°50′40″+90°−$\frac{1}{4}\frac{1}{4}$WM*				
Sum	2	13	31=WM=mCw			- - - - - -	2 13 31			
Diff.	0	21	37=SM=sCm			Cmw=82 4 39				
						Sum 180 0 0				

* The correction $\frac{1}{4}\frac{1}{4}$WM, by the above calculation, is 9′32″. The value made use of in finding Cwm is 8′50″, estimated by a rough calculation, and it was thought unnecessary to repeat the operation on account of this small difference.

In

In the triangle Cwm.

Cmw	82° 4′ 39″	Sine AC	0·0041651	- - - - - - - - - - - -				0·00417
Cw	3982	log.	3·6001013	- - - - - - - - - - - -				3·60010
Cwm	95 41 50	Sine	9·9978494	WM	2° 13′ 31″	Sine		8·58915
Cm	4000·51	log.	3·6021158	*wm*	156·1 miles		log.	2·19342

In the triangle Csm.

Cm+*Cs*	7982·51	log. AC	6·0978605	*Cms*	53°24′36″	Sine AC		0·09533
Cm−*Cs*	18·51	log.	1·2674064	*Cs*	3982	log.		3·60010
½sum ang.	89 49 11½	Tang.	12·5025138	*sCm*	0 21 37	Sine		7·79851
½diff. ang.	36 24 35½	Tang.	9·8677807	*sm*		31·2 miles	log.	1·49394

Sum 126 13 47 = *Csm*. ∴ Alt. at Weston 36° 13′ 47″.
Diff. 53 24 36 = *Cms*

The course from S to M is nearly N 3° W, the distance SM 21′37″, or 21 62′; hence the difference of latitude is 21·59′ = 21′ 35″, the departure 1·13′, and the difference of longitude 1·51′ = 1′ 31″

Latitude of Weston	41° 15′ 0″	Longitude of Weston	73° 27′ 0″
Diff. of latitude	21 35	Diff. of longitude	1 31
Latitude of Meteor	41 36 35 N	Long. of Meteor	73 28 31 W

Problem

Problem 2, *Fig.* 2.

Problem 2. Suppose that in two places, s, w, in given latitudes and longitudes, the angular elevations of a meteor Csm, Cwm, were observed, and the azimuth PSM at one of the places. It is required to find the situation of the meteor.

Solution.

This figure is to be marked like the first, then on SM (continued if necessary) let fall the perpendicular AW. Suppose a plane drawn through w, perpendicular to Cw, to cut the line sm in b. Join Cb cutting SM in B, and let CA continued cut sm in a. Find in the triangle PSW, the angles PWS, PSW, WSM (or ASW), and the side SW as in the last problem. Then in the right-angled spheric triangle SAW are given SW and the angle ASW, to find by spherics SA, AW, and the angle SWA. The angle Csm, or its supplement, is equal to the angle Csa, the angle sCa = arch SA, and the angle $Cas = 180° - Csa - sCa$.

$$\text{Sine } Cas : \text{Sine } Csa :: Cs : Ca$$

$$\text{Tang. AWB} = \frac{\text{Tang. } Cas}{\text{Sine AW}} \left(1 - \frac{Ca}{Cw} . \text{Cosine AW}\right)$$

The affection of the angle AWB may be determined by the figure and the data of the problem.*

$$\text{Sine MWB} = \frac{Ca}{Cw} \times \text{Cotang. } Cwm \times \text{Tang. } Cas \times \text{Cos. AWB.}$$

$$\text{MWA} = \text{AWB} \underset{\omega}{+} \text{MWB.}$$

The sign to be made use of is easily discovered by the figure, observing that the point B falls between M and S.

Cotang. mCw (= Cotang. MW) = Cosine MWA × Cotang. AW. The sum of the angles Cwm, mCw, subtracted from 180°, leaves Cmw. Then

$$\text{Sine } Cmw : \text{Sine } Cwm :: Cw : Cm.$$

The distances wm, sm, and the latitude and longitude of the meteor may be found as in Problem I.

Correction for refraction. When the distance of the meteor is great, the angles Csm,

* This angle may also be found in the following manner. Having in the plane triangle aCw, the sides Ca, Cw, and the angle aCw (= arch AW) the angles Caw, Cwa, may be found by plane trigonometry, and AWB by this formula. $\text{Tang AWB} = \frac{\text{Cos } Cwa \times \text{tang } Cas}{\text{Sine } Caw}$

Cwm,

Cwm, muſt be corrected for terreſtrial refraction, by ſubtracting one fourteenth part of the intercepted arches SM, WM reſpectively; but as theſe arches are generally unknown, it will be neceſſary (when great accuracy is required) to make the calculation with the altitudes uncorrected, and thus find the approximate values of the corrections of the altitudes for refraction, and, by repeating the operation, the required quantities will be obtained. In this way the refraction in the Wenham obſervations was found to be about nine or ten minutes.

Example.

Suppoſe the altitude of the meteor at the time of its diſappearance at Weſton was 75°, its azimuth at that place N15°W, and the correſponding altitude of the meteor at Wenham 5°30′ corrected for refraction. It is required to find the latitude and longitude of the meteor, its diſtance from Wenham and Weſton, and its vertical altitude above the level of the ſea. Exemplification of prob. 2.

Here are given the angle *Cwm*=90°+alt. at Wenham = 95° 30′, *Csm*=90°+ alt. at Weſton = 165°. The arch SW =2° 24′ 25″, the angle PWS = 125° 18′ 38″, and the angle PSW = 52° 56′ 34″, are found as in the laſt example. The angles PSW, PSM, added together, give WSM or ASW = 67° 56′ 34″.

This correſponds to Example 14, Table 1.

SW	2°24′25″	Tang. 8·6235990					Sine 8·6238157					Cot. 9·9996167
ASW	67 56 34	Cos. 9·5744474					Sine 9·9669004					Tan. 0·3923430
SA	0 54 16	Tang. 8·1982464	AW	2°13′50″	Sine 8·5967061			SWA	22°4′29″	cot. 0·3918597		
Csc	165 0 0	Sine 9·6129962	AW	2 13 50	Cot. 11·4094801							
Cas	14 5 44	Sin. AC 0·6134301	MWA	20 24 42	Cos. 9·9718374							
Cs	3982	log.*	MW	2 22 47	Cot. 11·3813275						Sine 8·61828	
Cm	3982	log. AC*	Cmm	95 30 0	Sine 9·9979960							
Ca ÷ Cm		log. 0·0264263	Cmm	82 7 13	sin. AC 0·0041201							0·00412
AW	2 13 50	Cos. 9·9996708	Sum	180 0 0								
	1·061933	log. 0·0260971	Cm	3982	log. 3·6001013							3·60010
Sub. 1	·061933	log. 8·7919271	Cm	4001·5	log. 3·6021174			mm	166·9	log.		2·22250
Cas	14 5 44	Tang. 9·3998470	Mm	19·5								
AW	2 13 50	sin. AC 1·4097939	MW	2°22′47″	Sine 8·6182788							
AWB	21 46 44	Tang. 9·6015630	MWA	20 24 42	Sine 9·5425303							
AWB	21 46 44	Cos. 9·9678392	AM	0 49 47	Sine 8·1608091							
Ca ÷ Cm		log. 0·0264263	SA	0 54 16								
Cmm	95 30	Cot. 8·9835769	SM	0 4 29	Sine 7·11533							
Cas	14 5 44	Tang. 9·3998470	Csm	165 0 0								
MWB	1 22 2	Sine 8·3776894	Cms	14 55 31	Sine AC 0·56912							
AWB	21 46 44		sum	180 0 0								
MWA	20 24 42		Cs	3982	log. 3·60010							
SWA	22 4 29		sm	20·2	log. 1·30455							
SWM	1 39 47											
PWS	125 18 38											
PWM	123 38 51 = calculated azimuth.											

The course from S to M is nearly N 15° W, the distance SM 4′ 29″; hence the difference of latitude of the points S, M is 4′ 20″, departure 69·6″, difference of longitude 93″. Hence the latitude of the point M is nearly 41° 19′ 20″ N, and its longitude 73° 28′ 33″. W.

If it be required to find the change in the above elements arising from an errour in the altitude at Wenham, it would only be necessary to repeat the latter part of the calculation, since the values of SA, SWA, AWB, would remain the same in both cases.

Remarks. When the distances between the observers of the meteors are small.

Remark 1. When the distances of the observers from each other and from the meteor are small, the correction arising from the spherical form of the Earth may be neglected, supposing the triangle SWM to be rectilinear, and drawn on a horizontal plane. In this case the calculations

* These logarithms are neglected, because one is the arithmetical complement of the other, and their sum (rejecting 10 in the index) is 0

will

will be rendered more simple, if the heights are estimated from a plane drawn through *w* parallel to the horizon, supposing the points *w*, W, to coincide. Then if the points, *s*, *w*, are at the same height, the points *s*, S, will also coincide as in fig. 3. If the points *s*, *w*, are not at the same level, as in fig. 4, the lines *ms*, MS (continued if necessary) will meet in S′, in the plane MWS, making SS′ = *Ss* × Cotang. alt. meteor at *s*. In either case there will be given, in Problem 1, the angles SWM, WSM, SMW (= 180°—SWM—WSM), and SW, to find by plane trigonometry WM, SM. Then the altitude of the meteor above the level of the point *w* will be represented by Prob. 1.

WM × Tang. elevation of the meteor at *w*.

In Problem 2, when the points *s*, *w*, are at the same level, as in fig. 3, there will be given the angles M*wm*, M*sm*, WSM, and the side SW, to find the angle SWM by the following formula. Prob. 2.

Sine SWM = Tang. alt. meteor at *w* × Cotang. alt. meteor at *s* × Sine WSM, which being found, the rest of the calculation may be made as above. When the points *s*, *w*, are not on the same level, as in fig. 4, the line SS′ being found as above, and the angle WSS′ equal to WSM or its supplement, SW being also given, the side S′W and the angles WS′S, S′WS, may be found by plane trigonometry. Then as before we shall have

Sine S′WM = Tang. alt. meteor at *w* × Cotang. alt. meteor at *s* × Sine WS′M, which gives the bearing of the meteor from W, whence the distance WM, and the height of the meteor as in Problem I.

Remark 2. When either of the azimuths or altitudes is not accurately known, but the limits between which the real value is contained are given; the situation of the meteor may be calculated for each of these limits, and by this means the limiting values of the required elements of the motion of the body may in general be obtained. This method is frequently made use of in this memoir. When either of the azimuths or altitudes is not accurately known.

Remark 3. In order to judge of the accuracy of the results obtained by the preceding problems, it will be useful to repeat the operation, making successively a small change in each of the given quantities. For if any one of the required quantities be not materially affected by these changes, Method of judging of the accuracy of the result.

the calculated value will in general be nearly correct. On the other hand, if a small errour in the observed angles produces a great errour in the result, it will be proper to reject it. Thus, in Problem 1, if the given angles WSM, SWM, are both very small, the least change in either of these angles will in general produce a great change in the situation of the point M, as is well known; and if the two places of observation *s*, *m*, are thus situate with respect to the meteor, the observations made at those places must not be combined together. This is the case with the observations of the Weston meteor made at Rutland and Weston, and for this reason the observations made at those places are not combined together, in the calculations made for determining the place of the meteor.

(To be concluded in our next.)

III.

On the method of JUSSIEU. *By* MRS. AGNES IBBETSON. *In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

Difficulties of a natural method.

PERFECTLY aware of the extreme difficulty of establishing a natural method in Botany, a task so *often tried without success* by our first botanists, even by Linneus *himself*; the difficulty of following nature, not only in the general appearances the superficial observer may mark, but tracing her in those minute and half *hidden* lines, which require the most *extreme attention*, the most profound knowledge in her ways, the most exact discrimination in her appearances; I cannot but deeply regret, that the long *stride* that excellent botanist Jussieu made, towards obtaining a *natural method*, should be (in any manner) *lost* to the public. But it should be considered, that the life of one man, nay of several, must be far *too short*, to bring to perfection a work of such magnitude; which requires the joint attention of many *physiologists*; who, as the science *advances*, should

cou-

constantly endeavour, to correct and improve *the plan*; and, applying to its aid every new light thrown on Botany, assimilate the knowledge of all in one grand undertaking.

When therefore we reflect how difficult the task is, it will surely be granted, that even Jussieu had not the *perfect* knowlege requisite to complete such a desideratum, which demands the most exact acquaintance chemically, botanically, and by dissection with every single plant. Drawings embracing most of the appearances are necessary; with the addition of a simple account of those peculiarities, which distinguish the class, the order, the genus, and species; pointing out the various marks, that ally a plant to each different family; mentioning the plants with which it naturally ranks, and *those traits* that form its artificial relations; explaining also its first rise form the *seed*, with all the peculiarities that attend its infant state. Its juices must *next* be *described*; the chemical properties of both *blood* and *sap*; the manner in which they flow; and any variation that may appertain to its *interior structure*; which is perhaps one of the most *curious parts* of a *plant*, and affords us an analogy never before sought. The formation of the flowers must be compared with those of the species not only *externally* but *internally*; on the latter of which *much* of the *habit* of a *plant depends*. The species of root must be shown, with its *interior* formation, and the direction of its strings; as well as the stem, and the variations to which it is subject. The account must mark the first shooting of its leaf and flower *bud*; and whether concealed or not within the *stem*; the effect of the *decayed leaf buds on the stem* in some plants, and the reason why it is not the same in all: the *shape* and *nature* of the full grown flower: when it opens, and closes, and at what times: the history of the *stamen* and *pistil*, and their manner of acting, &c.: *and above all*, each observation must be given in as few short *sentences* as *possible*; which will allow of easily comparing the plants together.

Requisites for this.

When all this is done, I doubt not the *real analogy* will appear: and that we shall then have a *natural method*, not fancied or drawn up by *man*; but *discovered* in *nature*, which will be followed with ease, and, like the *radii of a*

A natural method attainable.

circle branching off till it embraces in one *grand whole* all vegetable creation.

and should be sought after.

But while I point out the difficulties attending the formation of such a method, still the hope to attain it should not be *laid aside*; and though this great botanist failed in the foundation of *his plan*, he has evinced so much judgment and knowledge in the arrangement of his different *classes*, *orders*, and *genera*, that they may serve as a lesson to all those, who would attempt the task. I shall first therefore detail his method, show the defects of it, and then proceed to point out what appears to me to be the means of rendering it the foundation of future hopes and studies. Jussieu's plan was so simple, and so beautiful, that, while discovering the mistake, I could not but regret that it was not true, and admire the simplicity of the idea. He appeared to have brought together all those marks, both in *physiology* and *botany*, that should discriminate the different classes, all emerging from three great *centres*, appearing to arise as of *themselves* in the *order* and *genera nature would have dictated*. How distressing then to find all this *beautiful structure false*; I really felt deeply the necessity of undeceiving the botanical world, and awakening them from the admiration *the lovely fiction had caused*. Nor was Jussieu to blame; he is amply vindicated by the poorness of his magnifiers, by the knowledge of those times; though perhaps a little *blinded* by the *beauty* of his own *conceptions*. He examined with the powers he *had*; and, seeing all as others had *done*, he *looked no further*. I shall now give a sketch of his method, which will ever distinguish him as one of the first of philosophic botanists. It may be thought, that *I*, an obscure *individual*, am most *presumptuous* in daring to contradict such a genius: but it is not a matter of *argument* or *opinion*, but of *sight*, and having the benefit of better *microscopes* than he had, it is *these* that refute him, not I. He sought truth; and truth is my aim; nor can I believe if living he would not have wished the erroneous part of his method to be effaced.

Jussieu's method.

Jussieu conceived all plants to be separated into three grand divisions, or classes, by their cotyledons or seminal leaves

leaves: 1st, plants which had no cotyledons: 2d, plants which had one cotyledon: 3d, plants which had two. These were subdivided into orders, which took their distinction chiefly from there being or not being any albumen* in the seed; and this was followed by another separation into genera, by their seed being *farinaceous* or *fleshy*. Hence he proceeds to that strict and excellent separation, which is completely *in nature*, and *truly just*; "whether the stamens are fixed on the receptacle, on the calyx, or *on the pistil*," marking these by the appropriate names of "hypogenes, sur le réceptacle"; "perigenes, sur le calice"; "epigenes, sur le pistil". From this he proceeds to more minute divisions, explanatory of the species, in which he shows his knowledge with a precision and exactness impossible not to admire and feel. But in my last letter I showed, that these two last are the *only* circumstances of all the method, that have *truth for their basis*; for it is a positive fact, that there are no *certain number of cotyledones*; and though (except the firs) few plants show more than two above ground; yet they are within the seed, and their numbers are rarely regulated but by the length of time the seasons cause the seed to remain in the ground previous to its shooting. He took for cotyledons only the two *last leaves*, appearing from under the *heart*, and as two leaves must always be more or less *in that situation*, he saw not the rest, that were attached to it, or the number which surrounded it. The extreme smallness of the seeds of his *acotyledonous plants* required a nice hand to dissect them, and a *solar microscope* to enable the botanist to discern the figure: and his monocotyledons arose from the *mistake* of taking the *wrong leaf* for a *cotyledon*. As in all other seeds, the impregnating vessel must pass through

His mistake.

* As it may not be recollected or not known to the reader, that in my last letter on seeds I proved, that albumen and albumum are the same substance; I here repeat it; having taken the soft substance from a graft while joining, and from a seed; and compared it in every chemical way *possible* with so *small a substance*; it proved in every respect the same. Jussieu seems to be of this opinion: for in writing of both, he makes use of but one expression.

the

the corculum, to reach that recess in which most of the cotyledons are formed: in the *nonmammiferous alone*, palms, grapes, and what he calls the *aroides*, the cotyledon part need not pass under the heart but branch off; the cotyledon forming into leaves before the corculum, as in the grasses, or covering it as in the palms, while the primordial stalk alone passes through the heart. As to what he calls his dicotyledons, I do confefs I am astonished at his blindness, for in the walnut, mustard, oak, hazel, they are so large, that the naked eye can almost discern them, and see that there are more than two: and had I not been blinded by my perfect reliance on him, and assurance he must be right, I should not so long have shut my eyes against conviction. I have shown them to many, who were equally with myself convinced, that Jussieu's method was no longer tenable.

Distinction from the albumen in the seed.

I now come to that distinction, which rests on the seeds having or not having albumen. I showed in my last what albumen was in the opinion of the first physiologists, such as Duhamel, Mirbel, and Knight; and explained what I think it to be, *the softer part of the wood*, which when ready prepared for the sap vessels, they would shoot from any neighbouring part into the delicate matter, completing the formation, and it then becomes *perfect wood*, in every sense of the word; and by degrees hardens, and presses closer. There is scarce an experiment possible but I have made with this matter, treating it with *hot* and *cold* water, with spirit of wine, with a sand bath; and I could draw no other inference from its appearance, than the one I have given: which any person may prove the truth of by taking it from a *graft* of *three* or *four months* old before the plant shoots: now as the embryo is almost wholly wood, and the matter is unformed wood, how can some seeds be without albumen? it is impossible: The next part is, that the seeds should be farinaceous or fleshy. Seeds undoubtedly differ much in this respect, instead of being divided into two sorts only; they are separated by nature into four: appearing at first of exactly the same formed matter, though subject to such a variety of chemical changes, as is well worthy the attention of the learned chemist, who discovered

covered the manner of forming sugar in seeds, the truth of which secretion is every day exemplified: though all proceeds from the same apparent matter. As soon as the *embryo shoots*, all farinaceous seeds appear filled with a milky juice, which after a time hardens and becomes a white flower; as wheat, barley, clover, &c.: while in the diadelphian tribe the matter turns to a *deep green*, which hardens also: the 3d may be said to remain as it was at the beginning: while the 4th becomes an absolute oil: this last change appears to rise from the blood of the plant entering the seed for the production of the leaves, but the other variations are certainly most curious, and would make four divisions instead of two: as to the last, of epigenes and perigenes, there cannot be a more distinguishing or more appropriate mark.

I have now given a sketch of Jussieu's plan, and have also shown, that, the last two excepted, dissection proves his distinctions to be wholly false: but though this is the case, yet he has admirably selected his classes, orders, and genera, they will still stand as of themselves from their strong similitude to each other:—and enable us to continue our researches, and draw our conclusion, till farther examination may find a better basis to substitute for the false one: and as no plan of this sort can be perfected, till we have completed our acquaintance by dissection, as well as botanically, with every single plant; yet the nearer we approach to this knowledge, the sooner we shall attain our wishes; for I feel the most *perfect conviction*, that all the regularity found in every part must *arise* from a *natural method* established by the author of all order, and only to be discovered by extreme study and close attention to the *facts* to be found in the vegetable world.

There must be a natural method.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

I have just been cutting a graft of the daphne cneorum on the d. mezerium, where the formation of the albumen was not complete: and as in no plant this process is seen in a plainer

Grafts.

plainer manner, I had the delight of showing it to several gentlemen in various conditions: first without any vessels except the bastard pipes, and afterward a little lower with not only bastard pipes, but the silver grain complete, and the sap vessels: but this has been repeated in several grafts—indeed they need but be taken early enough, and they cannot fail to show this effect.

In the observations which I have mentioned as necessary to form a complete account of each individual plant, in the hopes of detecting the analogy necessary to produce the discovery of a natural method, I did not mean to find fault with the botanical description, for certainly Dr. S. has left us nothing to be desired on this head: but anatomically we may be said to be wholly ignorant of plants; this will undoubtedly produce an analogy closer and superior to all except a chemical analysis of its juices*:—to prove this I need give but a few; there is in most plants a place of separation to convey each different juice to that separate part of the flower it is intended to form and nourish. Linneus was of this opinion, and from the first dissections I made, I was convinced of the truth of the suspicion—I have since found it in almost every plant I could obtain, both exotic and native: some have this division under the pericarp, some at the bottom of the flower stalk, and the change is most conspicuous. There is also another curious particular—in all the phaseoli, and a few diadelphian plants, an increase of the joint. I noticed something resembling this in the mimosa sensitiva—but that had but one ball: but in the phaseolus coccinea, the elbow is not more completely jointed than the knots of this plant. The number depends on the smaller stalks shooting from the main stem, for to each there is a ball and socket. This ball turns in almost every direction; and if the large branch is cut, it will show it perfectly. I have seen as far as four balls, and the ease with which the peduncles turn will prove, that it must be something of this kind of formation: but what is most curious in this contrivance is, no sooner does the plant seed, than the ball decays—and if

Different juices conveyed to separate parts of a plant.

* From what we know already of the chemical analysis of vegetables, it is obvious, that no assistance in the classification of plants can be derived from this source. C.

it

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

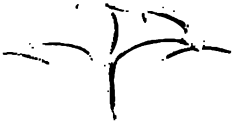


Fig. 6.

Fig. 7.

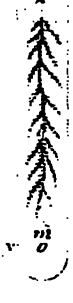


Fig. 9.

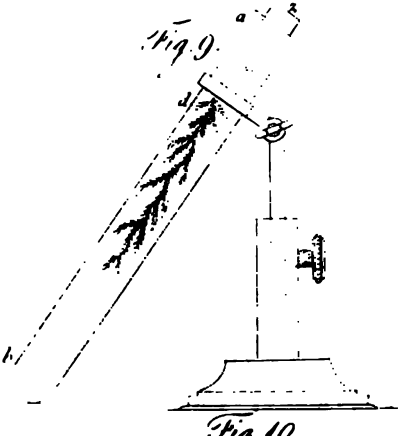
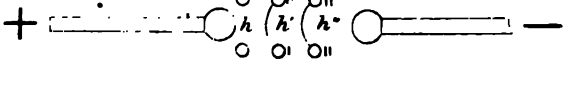


Fig. 8.

Fig. 10.



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it does not destroy the stem, this will *contract*, and will long after show the sockets empty.—I have added a figure of the dissection of the stem taken from a large plant of the phaseolus coccinea: see Pl. IV, fig. 1. I could mention many more proofs of curious inward structure, but shall confine myself to the different formation of the corolla, from which most part of what is called the *habit* of a plant *proceeds*. There are various species, *diversified* by their different *formation*, well worth a letter to show it, and as I mean in my next to give a complete sketch of the interior formation of corollas in general, I will not anticipate the subject.

IV.

Remarks on the British Species of Drosera. By ROBERT LYALL, *Surgeon, Member of the Royal Physical Society at Edinburgh, and Corresponding Member of the Literary and Philosophical Society at Manchester. Communicated by the Author.*

British species of drosera not well ascertained.

IN consequence of the contrary opinions of celebrated botanists, and of the slight variations to which the plants are liable, much ambiguity has been induced respecting our indigenous species of drosera. It is the object of the following remarks, to show by definite characters what should be reckoned distinct British species of this genus of plants.

As there can be no dispute, with regard to the d. rotundifolia, I proceed immediately to speak of the d. longifolia.

Linnæus had doubts of the d. longifolia.

Linnæus questioned whether the d. longifolia was a distinct species, or merely a variety of the d. rotundifolia; and Scopoli states, that the d. rotund. and longif. are only varieties of the same plant, and that he has various specimens, which show the gradation from the one to the other. Haller, on the contrary, will not allow, that these plants are merely varieties: indeed he considers them as distinct species;

Scopoli says it is merely a variety.

Considered as a species by Haller,

and many British botanists.

species*; and his opinion has been supported in Britain by Hudson, Lightfoot, Withering, Smith, and other botanists, if I may judge from the arrangement, which these celebrated men have followed in their different works. As I also concur in sentiment with Haller, it is my duty to mention the reasons which led me to adopt his opinion, and therefore I shall now present a statement of my observations, which was drawn up on the 17th of last July, a day on which I examined some hundred plants.

Reasons for acceding to this opinion.

Number of flowers on the scape of d. rotundifolia,

Of 120 plants of the d. rotundifolia from Paisley Moss,

1	had a scape which bore 2 flowers
4	..... 3 ditto
13	..... 4 ditto
19	..... 5 ditto
17	..... 6 ditto
26	..... 7 ditto
17	..... 8 ditto
16	..... 9 ditto
3	.....10 ditto
4	.....11 ditto
120	

Of the same number of plants of d. longifolia, likewise from Paisley Moss,

and of d. longifolia.

5	had a scape which bore 1 flower
20	..... 2 flowers
60	..... 3 ditto
28	..... 4 ditto
6	..... 5 ditto
1	..... 6 ditto
120	

Now, on comparing these tables, the difference in the number of flowers borne by the d. rotundifolia and longifolia must be obvious, for in the one, the scape for the most part supported from 4 to 9, in the other, only from 2 to 4 flowers.

Differences in the scape,

I shall in the next place notice the scape and leaves.

* See Miller's Dictionary.

From

From the scape of the d. rotund. being more slender, from the greater degree of curvature which the raceme assumes, from the greater number and smaller size of the flowers, than what is observed in the d. longifolia, I can at once tell, although I see no other part of the plant, when the scape is presented, to which of the species it belongs.

and in the leaves.

Beside the difference of size and form in the two species of drosera mentioned, it may be remarked, that in the *rotundifolia* the leaves in general spread almost horizontally, but in the *longifolia* they grow nearly erect: in the former the petiole or leafstalk is covered all over with long white hairs; in the latter species it is almost naked, a hair only occurring here and there, at considerable distances.

These differences not owing to the influence of soil.

Being well aware of the influence of soil in metamorphosing plants, I questioned whether the differences, which I have just detailed, between the d. rotund. and long. might not be produced by *soil* alone. On reflection, however, I was obliged to lay aside this notion, for the following reasons. 1st. In White Moss, near Manchester, and in the Paisley Moss, I have often seen the two species growing in contact with each other. 2. I observed the d. *rotund.* in Trafford Moss, and about Kersal Moor (both near Manchester) in very moist soil, though I do not recollect meeting with a single plant of the d. *longifolia.* 3. The plants of the d. longifolia, which bore the fewest flowers, were all dwarfish; showing, that, as the plant diminished in size, so did the flowers decrease in number. 4th. I have never been able, like Scopoli, to trace the gradation, from the one species to the other.

However nearly allied the d. *rotund.* and *long.* may seem at first sight, I think, from the reasons above assigned we are fully authorized, to reckon them distinct species: indeed had there been no other difference in the plants, except in the shape of the leaves, we might be justified in founding specific characters on this circumstance alone, according to the Linnean principles.

Specific characters.

The following specific characters are sufficient for a *Flora Britannica.*

Drosera rotundifolia. Foliis orbiculatis.

D. longifolia. Foliis oblongis.

I com-

D. anglica first distinguished by Hudson.

I come next to treat of the drosera anglica. Hudson I believe was the first who gave this species its trivial name, and who assigned the specific characters, by which we were to discriminate it from the d. longifolia, of which previous to his time it was only reckoned a variety.

Considered a species by Withering and Smith.

Withering and Smith seem to coincide in opinion with Hudson, for in their works, before noticed, they have followed his arrangement. It is therefore unwillingly, and with due deference to the authority of such superior botanists, that I venture to entertain a different sentiment.

Reasons for dissenting from this.

Among my last years notes, I find the following, "Is the d. anglica a distinct species, or merely a variety of d. longifolia? I have found plants corresponding to the description of the d. anglica given in Smith's Flor. Britan. &c., and with the figure of Morrison there referred to, which had only six styles and a three valved capsule."

In July last, I carried home a number of plants of drosera, some of which agreed with Morrison's first figure, and others with his second figure, of plate 4th, in size, shape, &c.; and when I examined the largest plants very particularly, I discovered, that one flower had six styles, and another nine, though they belonged to the same scape —that two flowers from another scape had each seven styles, —while another couple from a third scape possessed only six styles each. The styles I found also varied as to number in the smaller plants*.

Number of styles variable,

as well as that of the valves of the capsules.

On the uniformity of the valves of the capsules I could place no dependance; for in the large as well as the smaller plants they varied from three to four in number.

Not being able to perceive any difference in the other parts, as the leaves &c. of the different plants, except in size, I concluded, that what has been denominated the *dros. anglica* is *simply* a variety of drosera longifolia.

Only two British species.

If my conclusion then, be just, it follows, that we have only two British species of drosera, which may easily be discriminated by the characters before given.

Rorella of Ray.

Ray's *rorella longifolia maxima* justly belongs to d. longifolia, and is what was denominated d. anglica. His *rorella*

* It is worth noticing, that in the drosera *rotundif.* the number of styles is, likewise, subject to variation.

rotundifolia

rotundifolia perennis and *longifolia perennis* have, I am inclined to believe, been properly placed by Smith, Miller, &c. under drosera *rotundifolia* and *longifolia*; although I find myself unable to account for the statement given in his work, by Willisell, that some of the plants were annual, and others perennial. Is there no mistake in this account?

P. S. Since I finished these observations, I have procured Dr. Smith's small but excellent *Compendium Floræ Britannicæ;* and find that, after describing the d. anglica, the Dr. adds, "*Anne varietas?*" which plainly shows, that he entertained some suspicions, as to its being a distinct species. I hope my former remarks will afford sufficient answer to the question.

Dr. Smith questions the d. anglica's being a species.

N. B. It is proper to add, that many of my experiments, regarding the irritability of the drosera, related in vol. XXIV, p. 263 &c., of your Journal, were made upon the drosera anglica formerly so called.

V.

Remarks on the Expansion and Closing of the Flowers of the Passiflora cœrulea, or Passion-Flower. By the same.

SOME very curious and interesting phenomena present themselves, from the time that the flowers of the passiflora cœrulea, (passion-flower) begin to open, till the period at which they completely close. As I have nowhere met with a description of these phenomena, I have been induced to communicate a few observations relative to the subject.

Phenomena of the passion flower.

Before entering upon the immediate topic of this paper, I may remark, that the *corolla* of the passiflora cœrulea consists of five petals, and that the *calyx* is formed by five leaves, all of them terminated by crooked spines, which remain in contact, during the infantile state of the flower, or indeed till the flower is ready to burst. On the day that the flower is to open, all the divisions of the *calyx* begin either to part at once or alternately, and afterward expand considerably; when the petals of the *corolla* separate in a longitudinal

Its state before it opens.

Its mode of expanding.

longitudinal direction about their middle, and soon after disengage themselves at their *apices* and spring suddenly backward; with the *laciniæ* or divisions of the *calyx* accompanying them. Sometimes a number of petals disengage themselves and expand at once, at other times they proceed regularly to burst open one after another.

Changes in the stamens.

What deserves particular notice is the change, which the *stamens* and *pistils* undergo from the opening till the shutting of the flower.

Immediately before the flower opens, the *anthers* present their upper surface, which is covered with yellow *pollen*, toward the *stigmata*, as is shown in plate IV, fig. 2; being retained in that position by the closed petals. But when the petals, which stand opposite to their respective stamens, expand suddenly, which is generally the case, to a considerable degree, then the *anthers*, turning upon the top of their *filaments*, as upon an axis, appear as in fig. 3, with their yellow surface directed toward the heavens: and farther, as the petals gradually or suddenly expand to their full extent, so do the *anthers* become inclined downward, till they appear as in fig. 4, with their yellow surface facing the horizon. Very soon after, the anthers, turning still upon the same axis, acquire a very different appearance; for what was formerly their upper and yellow surface, now becomes directed to the earth. Lastly, the authers perform half a revolution (by means of a slender membranous string, which attaches them to the filaments described hereafter) and then appears as if suspended by a thread, fixed at their middle; their two ends pointing horizontally, as in fig. 5; whereas, before the complete expansion happened, one end of the anthers (see fig. 4) was directed almost perpendicularly upward and the other downward. The filaments, soon after this circumstance, become a little more curved, and the anthers retain the last mentioned position, till the time at which the flower begins to shut, (which for the most part happens in the course of 22 or 28 hours,) when it claims the attention of the investigating physiologist, to observe the other changes of situation, which the stamens undergo. After remaining nearly a day in the state represented by fig. 5, the *anthers*, by another semirevolution, regain nearly the same position as in fig. 4; the filaments then

then are intirely reflected, agreeably to the view of them in fig. 6. The upper surface of the anthers now is turned toward the columnar receptacle, shown at *a* in the same figure. Very often, however, the *anthers* remain as in fig. 5, when the filaments become recurved and approach the columnar receptacle.

The stamens, after undergoing the changes described, at last are surrounded by the closing nectaries (the pistils suffer an alteration hereafter to be noticed); the nectaries are next covered by the petals, and the petals by the calyx.—These different parts soon become flaccid, the flower withers, dies and lastly falls off.

Anterior to, or directly after the opening of the flower, the pistils* are somewhat erect, as is exhibited at fig. 2; but during the day they gradually decline more and more, till their stigmata hang among the anthers, as at fig. 5. In this state they continue for some time, and afterward either previous to, or during the closing of the flower, they retire by degrees, from among the anthers, and resume their pristine situation, as at fig. 2. Changes in the pistils.

To see the pistils in the different aspects noticed, fresh flowers only muft be examined; for when the flower fades, the corolla and calyx cease to afford support to the flaccid styles, which consequently fall downward.

I have referred above to the mode of connection between the anthers and filaments, which I shall now proceed to describe. The anther has on its back and middle part two small elevations, with a little cavity between them, from which a slender and very elastic membrane or ligament proceeds to be attached to the top of the filament, likewise between two eminences. This ligament, from its elastic nature and length, admits of motion in all directions. The anthers may be made to perform four or five revolutions before separating from the filament; for the ligament merely becomes twisted.—Hence we easily conceive, how the anthers perform the motions already spoken of. Mode of connexion between the anthers and filaments.

* I agree with those who refer the genus passiflora to the class and order pentandria trigynia, and therefore mean by pistils what Linnæus nominated styles.

Impregnation. *Impregnation.* Even when the flower first opens, the anthers are burst, and their surface is covered with pollen; but I imagine, that impregnation is not accomplished, till the period when the stigmata hang among the anthers, which, as I have formerly stated, happens during the *anthesis*, or time that the flower is expanded. If this be the case, we have certainly a very striking illustration of the manner, in which the *pistils* accommodate themselves to the *anthers*, in order that they may be fecundated; and consequently of the sexual system.

Have plants volition. Many disquisitions have been instituted, regarding the *volition* of plants; some admitting, while others deny, that they are capable of exerting such a *faculty*. Here I would wish to ask the abettors and opponents of both hypotheses, If the *passion-flower*, while performing the motions described, does not exhibit, as it were, a design to accomplish certain purposes by slow but regularly continued exertions—and of course an effort, at least, analogous to volition? At present I leave the determination of the question to others. Probably at a future period I may recur to this subject.

VI.

On the Influence of Galvanic Electricity in Metallic Arborizations: by C. J. THEODORE DE GROTTHUSS.*

Precipitation of one metal by another. *Sect.* I. THE precipitation of metals in solution, occasioned by the presence of certain other metals, exhibits such a striking analogy to the chemical effects of galvanic electricity, that we cannot avoid the idea of one and the same cause in all these phenomena.

Formation of the leaden tree. Let *z y*, Pl. IV, fig. 7, be a glass tube filled with an aqueous solution of acetate of lead; and *z* a piece of zinc, the lower end of which is immersed in the solution, and which is fastened so as to be retained at the top of the tube. Suppose the ramifications of the arbor saturni to have

Shown to be reached as far as *m*, I shall prove, that it increases by the

* Annales de Chimie, vol. LXIII, p. 5.

immediate

immediate superposition of the metallic particles on the ramifications most remote from the zinc. Suppose *om* to be a particle of oxide of lead in the lower part of the solution, not yet decomposed, and very near the ramifications: the moment the metal, *m*, is deposited on them, its oxigen, *o*, is consequently set free, and the oxidation of the zinc, *z*, at the same time goes on. Now it remains to be known, 1st. whether this atom of oxigen, *o*, be immediately seized by some peculiar impulse, that carries it through the liquid from *m* to *z*; and, 2ndly, how it is transported thither without any appearance of gas: or, 3dly, what becomes of it, if it be not moved out of its place. It would be absurd to suppose, that the oxide of lead, *mo*, came into contact with the zinc, *z*; and that the metal, *m*, after having yielded to it its oxigen, *o*, descended from *z* to *m*; since, in proportion as the arborization takes place in such a narrow tube, the superior part of the liquid no longer contains any lead, but acetate of zinc. Yet, as it would be easy to conceive some kind of flux and reflux, or to invent any other hypothesis, I will endeavour to obviate such suppositions by a decisive experiment, without entering into any tedious discussion.

by additions at the extremities.

Questions.

Untenable supposition.

If a tube, *a b*, fig. 8, be filled with a solution of muriate of tin diluted with water; and a little cylinder of zinc, *z*, be placed at the bottom of the tube; the arborization will go on regularly, often to the height of an inch or more, if the liquid be of a proper density. It is not till after these first rudiments of an arborization have taken place, that the different branches, crushed by their own weight, fall down, and exhibit a confused heap of metallic ramifications. The tree of Diana too grows very regularly from the bottom upwards, but this symmetry is essential, and the precipitation of the metal by superposition is sufficient to establish two propositions; namely, first, what becomes of the oxigen of the metallic particles, that are deposited on the extremities of the tree farthest from the zinc? and, 2dly, what becomes of the disoxigenized principle, at the place where it yields its oxigen to the zinc*? These two questions offer themselves in

Experiment.

Silver tree.

Propositions to be answered.

* We shall see presently, that, almost immediately after the arborization of the dissolved metal has commenced, nothing more is deposited about the zinc, which notwithstanding continues to be oxided; but all

all similar cases, and not alone in that before us. They who supposed in the first experiment, fig. 7, that the disoxigenized metal was conveyed instantaneously from *z* to *m*, or from the top downward; must necessarily admit, that in the second experiment the metal, *m*, after having ceded its oxigen, *o*, to the zinc, *z*, which is at the bottom, must have been carried from *o* toward *m*, that is, from the bottom upward. But these two suppositions are diametrically opposite to each other; and, besides, each is repugnant to the prime law of chemical attraction, according to which the sphere of perceptible affinity between bodies never extends beyond the distance of apparent contact. The absurdity of these hypotheses therefore is evident, and requires no farther discussion.

Erroneous suppositions.

Sect. II. There is another way of explaining metallic arborizations, more plausible indeed, but equally false.

Another hypothesis

It may be supposed, that, in proportion as the metallic particles of the solution are precipitated on the zinc, they are raised up successively by those, that proceed to be deposited on it in their turn, to cede to it their oxigen. In this case the first stratum of metal precipitated must be continually removing farther from that which acts as the precipitant; and all the various changes of form must take place solely in that part of the arborization, which immediately covers the latter metal. But we shall see the contrary to this demonstrated by experiment. If, for instance, the extreme ramifications of the tree of Saturn, fig. 7, have arrived as far as *c*, make a mark with ink opposite to this on the outside of the tube, and you will soon see, in a quarter of an hour's attentive observation, fresh ramifications implant themselves on those that before existed; the new accretions will advance beyond the mark, but the extremity *c*, corresponding to it, will remain fixed; and the ramifications most remote from the zinc will be continually undergoing changes of figure, while the part of the tree between the mark and the zinc will not experience the least change. The hypothesis alluded to therefore is untenable, but from the observations made we may infer, that, when a stratum

confuted by experiment.

the fresh particles of revived metal proceed to attach themselves at a distance from the zinc, that is at the last extremities of the arborization.

of

of metal in a solution is once deposited on that, which serves as its precipitant, it remains there immovable during the whole time of the experiment; and that the new metallic particles proceed to plant themselves on those, that already existed, so that the arborization increases only at its extremities.

Sect. III. This principle once established, we readily perceive, how the mutual attraction of the two metals, added to the predominant affinity of the one for the oxigen and acid of the other, fully explains, in most cases, the revival of the first particles, but not of the subsequent ones; and that the employment of all these forces is insufficient to solve the two principal problems. Still they have another defence, who suppose the attraction of one and the same metal sufficiently intense, to act the principal part in the precipitation of metals by one another. They might say, that, in proportion as the metal in solution is brought to deposit itself on that already precipitated, the oxigen of the former remains dissolved in the liquid, and the zinc slowly and gradually lays hold of it.

Supposition that the metal first revived attracts more.

Unfortunately the following experiment destroys these fine efforts of the imagination. Fill the tube *ab*, fig. 9, with an aqueous solution of muriate of tin, and immerse in it the zinc cylinder *z*; in the space of half an hour the cylinder will be covered with a fine tin arborization. Next separate these ramifications from the zinc, without destroying them: which may easily be done by dexterously turning the little cylinder: and let them sink gently as far as *c*, keeping them from falling quite to the bottom by giving the tube a proper inclination. It is essential, that the zinc remain immersed in the liquid, as in fig. 9. In this state the apparatus may continue hours, days, and even weeks, without the arborization *c d* receiving the least perceptible addition, though it is surrounded on all sides by oxide of tin in solution. If you have taken the precaution to pour water gently into the upper part of the tube, so that it does not mix with the muriate of tin occupying the lower part from *b* to *c*, the zinc may remain a long while in the water, without being covered with fresh ramifications of tin. Now if the attraction of one metal were sufficient to assimi-

Experiment to confute this.

late that which is in solution; and the oxigen, of which it is divested, remained in the liquid, to be gradually conveyed to the precipitant metal; the particles of tin ought to continue to be deposited on *c d*, as when this part was in contact with the zinc. But as no effect takes place, after this contact is broken off, this hypothesis is as unfounded as the preceding.

A contact of metals essential,

This simple experiment clearly demonstrates, that the metal in solution cannot continue to deposit itself on the extremities of the arborization *c d*, unless this communicates at some point with the metal *z*, which acts as the precipitant. But it is well known, that such a contact of two metals is sufficient, to give rise to a current of galvanic electricity; and that the effects of this electricity are identical with those observed in the precipitation of one metal by another: here then we have one proof, that this phenomenon depends on a similar cause. The following experiments fully confirm this conclusion.

which excites a current of electricity.

Farther experiments to show this.

Sect. IV. I filled half a tube six inches high, and a little more than a quarter of an inch in diameter, with a concentrated solution of nitrate of copper. Into the other half I poured very gently a solution, not very dense, of pure and perfectly neutral crystallized nitrate of silver. With the latter I had mixed a little alcohol, to diminish its specific gravity; and thus I was enabled to place the two liquids in contact without minghing them. That in the lower part of the tube was of a fine blue colour; that above was clear and colourless. In the middle of the latter, that is about an inch and half down the tube, I placed a small cylinder of copper, which soon began to precipitate metallic silver in the form of very delicate and fine garlands. These chains of silver flowers gradually approached the blue solution of copper, and, in twenty-four hours after they had reached it, I had the agreeable surprise to see their extremities covered with particles of copper, the colour of which was perfectly distinct, so that they could not be confounded with those of silver. The tree of Diana, which in this experiment appeared to have been transplanted into the garden of the Hesperides, acquired in four days an increase of a quarter of an inch, the colour

Arborizations of silver

tipped with copper.

and

and ramifications of which were unquestionably those of copper. This experiment I repeated, employing a cylinder of zinc instead of copper, and then the precipitation of the copper at the extremities of the branches of silver was much more speedy.

Arborization a superposition of particles effected by galvanism.

Hence it evidently follows, 1st, that the arborization of metals, precipitated in the perfect state from their solvents, is formed by superposition of their particles on one another: 2dly, that, since neither copper nor silver has of itself the power of reviving the metal contained in nitrate of copper, these two metals were rendered capable of producing this effect only in virtue of a current of galvanic electricity, resulting from their mutual contact.

Farther proof.

The following is a farther proof of the truth of this induction. Establish a communication between a plate of copper and a plate of silver, both well polished, by one of their extremities, and immerse the other two extremities in a solution of my salt of copper, in such a manner that there shall be the space of an inch between them. After leaving them thus four and twenty hours, the silver will be distinctly covered with a coat of copper, and the other metal will be oxided*. To render this effect still more evident, I place a few particles of nitrate or sulphate of copper, slightly moistened, between a silver coin and a plate of copper, both previously well cleaned with acid. I then tie them, or compress them together, and leave them for a few hours. On separating them, the silver will be found covered with a coat of metallic copper, very distinguishable by its colour. It is a curious fact, that a salt with an earthy or alkaline base may be employed instead of a salt of copper; but in this case it is obvious, that the acid of the salt first takes up a little of the copper, and then depo-

Silver coated with copper.

An earthy or alkaline salt between the two metals will answer.

* Dr Wollaston, who first observed the chemical action of two metallic plates immersed in solutions of different metals, supposed, that one of them must possess the property of precipitating the metal in solution, in order that the other might coat itself with it also by means of a conducting power. The experiment I have related however proves, that this is not a necessary condition; and we may obtain in a few minutes the revival of zinc, by placing the sulphate of this metal moistened with water between a plate of zinc and a plate of copper.

It is not necessary that the metal in solution should be precipitable by the other.

sits

sits it on the silver. It follows from these facts, that the galvanic electricity excited by the contact of two metals is sufficient, to effect the precipitation of a metal held in solution, which would not have been precipitated by either of the metals taken separately.

Theory of the galvanic decomposition of water.

Sect. V. It now remains to solve the two problems in Sect. I. Perhaps the reader will have already remarked, that they are nearly the same as present themselves in the galvanic decomposition of water, and that they may be all solved on the same principles. On this account I think it necessary to recapitulate in a few words the theory I gave of the latter phenomenon in a short paper in the Ann. de Chim. for April, 1806. I first proved, that water, traversed by a current of galvanism, becomes oxigenized at the positive pole, and hidroguretted at the negative pole. Though the contrary has been presumed; and though it has been said, that it becomes neither oxigenized nor hidroguretted; my recent experiments appear to me evidently to confirm the principle I have announced. I will relate only two.

Proofs that the water is oxigenized at the positive, and hidroguretted at the negative pole.

a. Hidroguretted sulphuret of barytes, exposed to the action of the pile, affords gradually round the positive pole an abundant quantity of a white precipitate, which has all the characters of sulphate of barytes; while the liquid round the negative pole becomes evidently clearer, and ultimately colourless.

b. If the galvanic current traverse an alkaline solution of indigo, made by means of some disoxigenizing substance, immediately there is formed at the zinc pole a precipitate of a fine blue colour, which is capable of being completely redissolved, if the respective places of the two poles be changed*.

Water, when galvanized, becomes a secondary pile.

Sect. VI. Water becomes a real secondary pile while it experiences the galvanic action. On this occasion we may recollect the remark of Volta, that a piece of wet paper, each extremity of which touches one of the poles of the pile, retains for some minutes the charge received, even

* In these trials I have never immersed the poles, by which term I mean the conducting wires, more than four or five lines below the surface of the liquid.

when

when it is no longer in contact with the apparatus. Pure water in a curved tube, which has been placed in the galvanic circuit, likewise retains for a short time the power of contracting the muscles of a prepared frog. I conceive therefore, that its galvanic decomposition is effected in the following way. Let us suppose a thread of water formed of three integrant particles, fig. 10, each composed of two atoms of oxigen, represented by $\overset{o}{o}$, and one of hidrogen, represented by h: and let it be observed, that what is said of this thread may be afterward applied to any quantity of this fluid.

Mode in which its decomposition is effected.

The moment a galvanic current is established, the electric polarity manifests itself between the elementary particles of the water, so that they appear to be the complement of the pile in action. Now since the oxigen is attracted by the + or zinc pole, and hidrogen by the — or copper pole, we are naturally led to infer, that each of the two principles requires, at the instant of its production, an electrogalvanic state opposite to the poles which attracts it; and as this reasoning may be extended to all the particles of water in the direction of the galvanic current, it follows, that, if we take them two by two, the homogeneal principles will repel each other, and the heterogeneous principles attract each other, alternately*. Thus as soon as the oxigen, $\overset{o}{o}$, passes to the state of elastic fluid in consequence of the attraction of the pole endued with vitreous electricity, its hidrogen h, by virtue of this electricity, repels its similar h', and enters into combination with the oxigen, $\overset{o}{o}'$, of the adjacent particle; at the same time h', being repelled, transmits its movement to the homogeneous atom, h'', anditse lf unites with the oxigen, $\overset{o}{o}''$, of the third particle, the hidrogen of which, h''', must finally assume the state of gas, since it is in immediate contact with the pole charged with resinous electricity. In the whole of this process, it appears, the particles of water at the extremities of the conducting thread are alone decomposed, while the intermediate ones reciprocally change their component principles in succession, but without altering their nature. This theory agrees

* The idea of this polarity, analogous to that which exists in the pile, I conceive distinguishes the explanation I here give from any other.

completely

completely with the facts, which prove beyond question, that the water becomes oxigenized at the positive pole, and hidroguretted at the negative: for, since the oxigen of the whole quantity of water traversed by the galvanic current tends incessantly, to proceed toward the pole endued with vitreous electricity; and the hidrogen is similarly affected with regard to the wire charged with resinous electricity; it evidently follows, that the water must become oxigenized at the zinc pole, and hidroguretted at the copper pole.

This consistent with the laws of chemical affinity.

Sect. VII. In this theory of a chemical phenomenon every thing is conformable to that law of affinity, by which the radius of its sphere of action never extends beyond the distance of apparent contact. The effects of this attractive power however may become sensible at a distance, when it is transmitted by *dissimilar* particles; as the effects of repulsion may be by the transmission of the corpuscular action by *similar* particles. Elastic bodies, that possess a certain mobility analogous to that of the particles of a liquid, afford a striking example of a transmitted motion, which does not become sensible, till it has reached a certain distance from the point of its origin. If a ball of ivory, suspended by a thread, be let fall against a row of similar balls, suspended in the same manner, and in contact with each other, the impulse given to the first will be immediately communicated to the second, from which it will be transmitted to the third, and so on to the last ball; which, receiving the motion, and having nothing to communicate it to, will fly off with a force equal to the primary impulse. This well known law of mechanics may be happily applied in all chemical phenomena, where the object is to explain a force, the effect of which is become sensible at a distance.

Example of transmitted motion in mechanics.

Theory of the precipitation of one metal by another.

Sect. VIII. The theory of the galvanic decomposition of water, which I have just explained, is that of the precipitation of metals by one another. With regard to the revival of the first particles of the metal held in solution, I shall offer but one remark in the next section, since Berthollet and Vauquelin have already treated the subject in such a superior manner*: but from the facts and reasonings

* Essay on Chemical Statics, vol. II, p. 421; and Annales de Chimie, vol. XXVIII.

given

given above, it is impossible to doubt, that the subsequent metallic arborization is owing to an electrogalvanic polarity, which is established between the elementary particles of the liquid comprised between the precipitant metal and the extremities of that which is precipitated. The oxidation and revival manifest themselves in this process at a distance more or less considerable, according to the progress of the arborization; while all the intermediate component particles of the liquid, in consequence of this polarity, undergo a mutual exchange of their elementary principles. If it were required to render visible the positive electricity of the precipitant metal, the extremity z' of which, fig. 7, might be made to communicate with a sensible electrometer; it should be remembered, that this is impossible, because the metal is immersed in the liquid, and the revival of that in the solution is effected in consequence of the galvanic circle being then closed, so that it is in the state of a pile, the two poles of which communicate by means of a conducting substance. The fine experiments of Volta show, that the contact of two metals is sufficient, to excite a current of electricity; and all that illustrious philosopher demonstrated with plates is perfectly applicable in this case to the elementary particles of water. It is sufficient therefore, for the complete solution of the two propositions in my first section, to conceive the idea of an electric polarity established between these particles as often as they are attracted by polar electricity*.

Why the electricity does not affect the electrometer.

Sect. IX. Alcohol containing any metallic salt, as acetate of lead for instance, powerfully opposes the precipitation of the metal that is held in solution, whether a piece of zinc be left in it, or it be galvanized. This appears to me to prove, that the elelectric polarity manifests itself between the component particles of the *water*, and not of the *oxide*, as I was myself inclined to think some time ago. At present I am of opinion, that the precipitant metal acquires the positive state by contact with the oxigen of the water, and that it attracts this oxigen from being in the opposite

Alcoholic solutions of metals not precipitated like aqueous.

* This expression appears to me well adapted to designate galvanic electricity.

state:

state: the hidrogen being on the contrary repelled, because it also acquires the vitreous electrity, attracts for the same reason the oxigen of the metallic oxide held in solution by the acid and water; whence it follows, that the metal, being divested of its oxigen, must reappear in its perfect lustre.

Quantity of caloric in an oxide probably proportionate to the oxidability of the metal.

A circumstance not to be forgotten in this explanation, but to which I cannot at present assign its just value, is the variable quantity of caloric retained by metallic oxides. It appears probable to me, that this quantity is in the inverse ratio of the oxidability of the metal. The following is the manner in which Berthollet expresses himself on this head in his excellent Essay on Chemical Statics, vol. II, p. 382.

Berthollet's remark

"Oxigen retains a larger or smaller quantity of caloric in its combination with metals, as well as in those it forms with other substances. On this depend some of the properties, that distinguish oxides in their relation to combustible substances. Those of gold, silver, and mercury, retain a great deal; whence their compounds with ammonia detonate either by a slight rise of temperature, or even by compression. The oxide of copper, which also is rendered capable of decomposing ammonia by an increase of temperature, produces however no detonation; which shows, that the oxigen in it is much more deprived of caloric. The oxide of silver detonates more forcibly than that of gold, or of mercury; so that the oxigen appears to retain more caloric in the former, than in the latter."

questioned by Brugnatelli.

Brugnatelli, whose sagacity is generally acknowledged, objects to this theory, that we observe no evolution of caloric when silver is revived by means of copper; and thence he concludes in favour of his own theory of thermoxigen. This observation appeared to me so decisive, that I had completely adopted the opinion of Brugnatelli in this respect.

Evolution of caloric not always perceptible:

However, reflecting one day on this interesting subject, I recollected, that a thermometer, placed under a receiver in which a vacuum is made, is but little affected by the cold produced by the dilatation of the air, and it occurred to me, that the caloric, which is extricated in the precipitation of one metal by another, may be so quickly absorbed by the surrounding bodies, as not to become sensible. Hence I conceived, that its dispersion might be prevented by diminish-

ing

ing the bulk of these bodies. To prove this, I made a saturated solution of pure nitrate of silver, crystallized and perfectly neutral, in distilled water; and with this solution I filled a very thin tube, 108 mill. [4·25 inches] long, and 7 mill. [2·75 lines] in diameter. After satisfying myself, that the solution occasioned no change in blue paper, I let fall to the bottom of the tube a small cylinder of well cleaned copper, 14 mill. [5.5 lines] long, and 7 mill. [2·75 lines] in diameter. Immediately the cylinder was covered with a forest of Diana's trees, and on touching the corresponding part of the tube the heat was very sensible. Having substituted a zinc instead of a copper cylinder in another experiment, the effect was much more considerable; and what appeared to me very singular was, that, in proportion as the bulk of the silver arborization increased, I was obliged to move my fingers towards its extremities, to be able to feel the heat. These experiments appear to me to prove, that the quantity of caloric evolved in the precipitation of metals by one another is equal to the difference that exists between the caloric that was contained in the oxide of the metal revived, and that which is absorbed by the oxide of the precipitant entering into solution.

but it is in the revival of silver by means of copper in a thin slender tube.

Sect. X. It only remains for me to remove two difficulties, that may be objected to the preceding theory, which embraces almost all the chemical effects of galvanic electricity. I proceed to the first.

Two objections to the theory.

a. It has been observed, that, when the two poles are immersed in separate glasses filled with water, no gas is evolved, because the communication necessary for the discharge of the pile is interrupted. To remove this obstacle, we have only to immerse two fingers of the same hand in the two glasses, and immediately the gasses will appear. This observation has been the stumbling block of all theories, as it was highly puzzling to explain, 1st, what became of the hidrogen in the glass in which the zinc pole evolved pure oxigen; and 2dly, what on the other hand became of the oxigen in the glass in which the copper pole equally evolved pure hidrogen. A little sagacity, and a clear idea of the establishment of the electrogalvanic polarity between the elementary particles of water, will solve this first difficulty,

The 1st answered.

so

so far as to find in it a new confirmation of the truth of the theory here given. Let us first take the simplest case, and suppose, that we have established the communication between the two glasses of water by means of a piece of wet paper or linen. It is evident, that in this case the water contained in these two substances alone produces the communication; since the linen and paper, when thoroughly dry, comport themselves as nonconductors, or at least as badly conducting substances. Thus there is nothing here to perplex us, except a delusive appearance; and every thing ought to go on, as if the poles were immersed in a single vessel of water. Now let us substitute our fingers instead of wet paper, and consider, that all the muscular parts of an animal are impregnated with an abundant quantity of aqueous fluids, the elementary principles of which are equally capable of acquiring the galvanic polarity, and consequently experiencing the same successive exchange as those of the water contained in the paper. These considerations are sufficient to show, that, in the experiment which at first sight appeared incompatible with our theory, every thing is in reality conformable to it. The property of almost all imperfect conductors impregnated with moisture, to become secondary piles when they are placed in the circuit of the secondary pile of Volta, proves the real existence of the electrogalvanic polarity between the particles of water: but what evinces, that the communication between the water in the two glasses is established solely by the aqueous humours with which our organs are impregnated, is, if we substitute for these organs a dry substance, though a good conductor, as a wire of gold or platina, the developement of hidrogen and oxigen both will immediately take place in each of the two vessels.

2d objection

b. The second and much inferior difficulty consists in accounting for the difference perceived between the decomposition of water produced by an electric machine, and that effected by the pile. In the first the two principles of water, according to Dr. Wollaston, are always evolved mixed together; while the pile separates them in a pure state, and at a distance from each other. The reason of this no doubt is, in the electromotive apparatus the two electric fluids are immediately in contact, whence follows an uninterrupted

answered.

rupted electric polarity, capable of generating a similar polarity between the elements of water; while in all electrical machines, and even in the Leyden phial, the two electricities are always separated from one another by an insulating body, as glass, air, resin, &c. Insulated electricity always acts at a distance, and glides with great swiftness over the surface of conducting bodies: the galvanic fluid on the contrary, which might be more appropriately termed polar electricity, acts on the same substances only when in contact, and by transforming them into secondary piles. This analogy of action at a distance and in contact is observed likewise between the attraction of the heavenly bodies and chemical affinity; which is the more remarkable, as there is no inconsistency in considering the light, that proceeds from those stars that form a part of the universe, as an effect of electricity.

Gravitation compared with chemical affinity.

It appears to me probable, that, in the galvanic decomposition of water, each of the two component principles of this liquid combines intimately with the electric fluid of that pole, by which it is attracted; and that they are both converted into the elastic state in consequence of this combination. If this were demonstrated, the light produced in the combustion of hidrogen gas would also be explained by the prompt reunion of the two electric fluids set free.

When water is decomposed its principles combine with the electric fluid.

Hence the light on burning hidrogen.

VII.

Observations on the Effects of Magnesia, in preventing an increased Formation of Uric Acid; with some Remarks on the Composition of the Urine. Communicated by Mr. WILLIAM T. BRANDE, F.R.S., *to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society.**

MR. Home's inquiries into the functions of the stomach, and his discovery of liquids passing from the cardiac

Calculous complaints

* Abridged from the Phil. Trans. for 1810, p. 136.

avoided by preventing the formation of uric acid.

diac portion into the circulation of the blood*, led him to consider, that the generality of calculous complaints might possibly be prevented by introducing into the stomach such substances, as are capable of preventing the formation of uric acid; and that this mode of treatment would have many advantages over the usual method, which consists in attempting to dissolve the uric acid after it is formed.

Magnesia adapted to this purpose,

He consulted Mr. Hatchett on the substance most likely to produce this effect, and asked if magnesia, from its insolubility in water, was not well adapted for the purpose, as it would remain in the stomach, until it should combine with any acid, or be carried along with the food towards the pylorus.

Mr. Hatchet knew of nothing more likely to produce the desired effect; and on putting this theory to the test of experiment, it was found by a very careful examination of the urine, that in several instances, where there was an increased formation of uric acid, magnesia diminished it in a much greater degree, than had been effected by the use, and that a very liberal one, of the alkalis, in the same patient.

as shown by experience,

This circumstance led Mr. Home to wish for a more complete investigation of the subject, and he requested me to assist him in the prosecution of it. Since that time many opportunities have occurred of carrying on the inquiry during an attendance on patients, labouring under calculous complaints.

on several occasions.

It is proposed to lay the results of our joint labours before this Society, with a view to establish a fact of so much importance in the treatment of those diseases.

The four following cases include the principal varieties of the disorder, which have been met with, and are therefore selected from among many others, to prevent unnecessary repetitions. In each of them the urine was occasionally carefully analysed.

1st case.

Case I. A gentleman, sixty years of age, who had been in the habit of indulging in the free use of acid liquors, had repeatedly passed small calculi composed entirely of uric acid; his urine, immediately after being voided, de-

* Philos. Trans. 1808. See Journal, vol. XX, p. 374, and XXI, 103.

posited

posited at all times a considerable quantity of that substance, in the form of a red powder, and occasionally in larger crystals.

Nine drachms of subcarbonate of soda, dissolved in water highly impregnated with carbonic acid, and taken in the course of the day at three doses, appeared to have no effect whatever on the formation of uric acid; the red sand was deposited as usual, and the small calculi continued to form.

On account of the inefficacy of this medicine, he was advised to try the vegetable alkali, and three drachms of subcarbonate of potash dissolved in water, slightly impregnated with carbonic acid, were taken at similar intervals.

The deposit of uric acid in the urine was now somewhat diminished; but during this free use of alkalis, which with little interruption was persevered in for more than a year, the small calculi still continued to be voided.

The magnesia was then tried, fifteen grains being given three times a day in an ounce and half of infusion of gentian. In a week the uric acid was found to have diminshed in quantity, and after the first three weeks it was met with but occasionally. The use of the magnesia was persevered in for three months, during which time no calculi were voided, and there was no material deposit in the urine.

This patient was extremely subject to heartburn, and he likewise complained of a sense of weight and uneasiness about the region of the stomach, both of which symptoms have disappeared.

Case II. A Gentleman, about 40, during four years had occasionally voided considerable quantities of uric acid, in the form of red sand, and had once passed a small calculus. He took carbonate of soda dissolved in water highly impregnated with carbonic acid for some time; to which *pure* potash was afterward added: but with little effect. Recourse was then had to magnesia, twenty grains of which, taken night and morning, for six weeks, removed all the superabundant uric acid, though he put himself under no unusual restraint in his mode of living. Case 2.

Case III. A gentleman, 43 years of age, after taking violent exercise on horseback, was seized with pain in the Case 3.

right

right kidney and ureter. In the course of the night he passed a small uric calculus. Having had recourse to soda water with little benefit, he took twenty grains of magnesia every night. His urine not being quite free from red sand in the course of three weeks, he took an equal dose of magnesia every morning, and in a month after this his urine was perfectly healthy. In about three months the uric acid again made its appearance in the form of red crystalline sand, and was again removed by the use of the magnesia night and morning for three weeks.

Case 4. *Case* IV. A gentleman, aged 56, after recovering from a severe fit of the gout, voided constantly a large quantity of mucus in his urine, a symptom which he had never before noticed. There was also, occasionally, abundance of red sand, consisting principally of uric acid, but he had never voided a calculus.

His stomach was uncommonly weak, he was often affected with heartburn, and an almost constant pain in the neighbourhood of the right kidney. He had been in the habit of taking tincture of bark, and other spirituous medicines, from a belief, that the pain in his right side arose from gout in the stomach.

He had already attempted to use the alkalis, which had produced such unpleasant sensations in the stomach, that he could not be prevailed upon to try them again in any form.

Under these circumstances, he readily acceded to a new plan of treatment. He was directed to omit the use of spirituous medicines, and take twenty grains of magnesia, three times a day, in water; but this operating too powerfully upon the bowels, the same quantity of magnesia was taken twice a day only, with an addition of five drops of laudanum to each dose.

This plan was pursued without intermission for three weeks, and he received considerable benefit, as far as concerned the state of the stomach, and pain in the region of the kidney. The urine, which was examined once a week, was also, on the whole improved, but it occasionally deposited a very copious sediment, consisting of uric acid, with a variable proportion of mucous secretion.

After

After a farther continuance of the use of the magnesia for three weeks, the urine was often much loaded with uric acid and mucus; but these appearances, which before the use of the magnesia were constant, are now only occasional, so that the disposition to form a redundant quantity of uric acid is much diminished: it is also deserving of remark, that there has not been the slightest symptom of gout from the time of the last attack, which is more than a year back, a longer interval of ease, than this patient has experienced for the last six years.

He has now omitted the regular use of the magnesia, but on perceiving any unpleasant sensation in the stomach, he returns to it for a week or ten days, and then again leaves it off.

From the preceding cases it appears, that the effects of magnesia taken into the stomach are in many respects different from those produced by the alkalis in those patients in whom there is a disposition to form a superabundant quantity of uric acid. Remarks.

With a view to ascertain their comparative effects on healthy urine, when taken under the same circumstances, the following experiments were made.

Experiment 1. *On Soda.*

Two drachms of subcarbonate of soda were taken on an empty stomach at nine o'clock in the morning, dissolved in three ounces of water, and immediately afterward a large cup of warm tea. Action of subcarbonate of soda on the urine.

In six minutes, about one ounce of urine was voided; in twenty minutes six ounces more; and after two hours, a similar quantity.

The first portion became very turbid, within ten minutes after it had been voided, and deposited a copious sediment of the phosphates, in consequence of the action of the alkali upon the urine. It slightly restored the blue colour to litmus paper reddened with vinegar: the alkali therefore was not merely in sufficient quantity to saturate the uncombined acid in the urine, and consequently to throw down the phosphates, but it was in excess, and the urine was voided alkaline.

The urine voided after twenty minutes also deposited a cloud of the phosphates; but the transparency of that voided two hours after the alkali had been taken was not disturbed.

Here, therefore, the effect of the alkali upon the urine was at its maximum, probably in less than a quarter of an hour after it had been taken into the stomach, and in less than two hours, the whole of the alkali had passed off.

Experiment 2. *On Soda, with excess of Carbonic Acid.*

Action of the supercarbonate of soda on the urine.

The same quantity of soda, dissolved in eight ounces of water, very highly impregnated with carbonic acid, was taken under the same circumstances as in the former experiment, and the urine was voided at nearly similar intervals.

The separation of the phosphates was less distinct, and less rapid. In two hours after the urine had been voided, there was a small deposit, composed principally of phosphate of lime; there was also a distinct pellicle on the surface, consisting of the triple phosphate of ammonia and magnesia. This appearance, produced by the escape of the carbonic acid, which had before retained the ammoniaco-magnesian phosphate in solution, and which now occasions its deposition on the surface, is by no means uncommon, even in the urine of healthy persons: in the present instance, it appears to prove, that carbonic acid passes off from the stomach by the kidnies, for after taking the alkalis in water very highly impregnated with it, the pellicle is uniformly produced, and is also much more abundant and distinct, than under any other circumstances.

Potash similar in its action.

In similar experiments with potash, the results were in all cases as similar as could be expected in researches of this nature.

Experiment 3. *On Magnesia.*

Action of magnesia on the urine.

Magnesia was taken under circumstances similar to those of the soda in the former experiment: in the quantity of half a drachm, it produced no sensible effect upon the urine during the whole day. When taken in the dose of a drachm

drachm at nine o'clock in the morning, the urine voided at twelve o'clock became slightly turbid: at three o'clock the effect of the magnesia was at its maximum, and a distinct separation of the phosphates took place, partly in the form of a film, which when examined was found to be the triple phosphate of ammonia and magnesia, and partly in the state of a white powder, consisting almost entirely of the triple phosphate and phosphate of lime.

The effect of large doses of magnesia, in producing a white sediment in the urine, is very commonly known, and has been erroneously attributed to the magnesia passing off by the kidnies.

These experiments show, that magnesia, even in very large doses, neither produces so rapid an effect upon the urine, nor so copious a separation of the phosphates, as the alkalis; on this its value as a remedy in calculous disorders seems materially to depend.

Experiment 4. *On Lime.*

Two ounces of lime water, taken in the morning upon an empty stomach, with a cup of milk and water, produced no effect whatever. Action of lime on the urine.

A pint of lime water, taken at four intervals of an hour each, produced a slight deposition of the phosphates at the end of the fifth hour. The urine voided at the third hour was not at all affected; at the fifth hour, the effect appeared at its height, but was not nearly so distinct as from small doses of soda, notwithstanding the insoluble compounds, which lime might be expected to form with the acids in the urine.

The unpleasant taste of lime water, the quantity in which it requires to be taken, on account of the small proportion of the earth which is held in solution, and the uncertainty of its effect, are circumstances which render it of little use, excepting in some very rare cases, where it has been found to agree particularly well with the stomach.

The effect of carbonate of lime upon the urine was much less distinct than that of lime water: at times it produced no effect, but when taken in very large doses, a slight deposition of the phosphates was produced. Action of chalk on the urine.

The results were uniform in three individuals.

These experiments were repeated upon three different individuals, and there was always a uniformity in the results.

When the medicines were taken some hours after food being received into the stomach, their effects upon the urine were retarded, but not prevented.

Other substances tried.

The effects of many other substances upon the urine were examined into during this investigation, but they varied so much according to circumstances, that no satisfactory results were produced.

As it is found in the foregoing experiments, that the effects of soda on the urine are modified by the presence of carbonic acid, the following experiment was made, to ascertain, whether any sensible effects are produced by that acid on healthy urine.

Action of carbonic acid on the urine.

Twelve ounces of water very highly impregnated with carbonic acid were taken upon an empty stomach at nine o'clock in the morning. At ten o'clock about eight ounces of urine were voided, which had a natural appearance; but, when compared with urine voided under common circumstances, was found to contain a superabundant quantity of carbonic acid: this gas was copiously given off when the urine was gently heated, or when it was exposed under the exhausted receiver of an air pump.

Sometimes useful.

In a patient who had a calculus of large dimensions extracted from the bladder, composed entirely of the phosphates, and whose stomach did not admit of the use of stronger acids, carbonic acid was given in water; it was found peculiarly grateful to the stomach, and upon examining the urine during its use, the phosphates were only voided in solution, but when at any time it was left off, they were voided in the form of white sand.

VIII.

Observations and Experiments on the Nature of Oximuriatic Acid, and its relation to Muriatic Acid, by J. MURRAY, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR.

Chemical theory liable to change.

THE theories of chemistry, resting on evidence not strictly demonstrative, are liable to frequent modification from

from the progress of discovery. Opinions however more unexpected have seldom been announced to chemists, than those lately advanced by Mr. Davy with regard to the constitution of muriatic and oximuriatic acids—that the latter is not a compound of muriatic acid and oxigen, but a simple substance, and that the former is a compound of this substance with hidrogen. The more general principle connected with these opinions—that oximuriatic acid is, like oxigen, an acidifying element, which forms an extensive series of analogous compounds with inflammables and metals, leads still more directly to the subversion of the established chemical systems, and to an entire revolution in some of the most important doctrines of the science.

Opinions of Mr. Davy on the muriatic & oximuriatic acids

important as introducing a new acidifying principle,

Opinions so novel and important claim attention; and this is not less due to them from the authority on which they are advanced. They derive too an additional interest from their connection with a part of chemical theory at present involved in much obscurity—the relation of water to the constitution of some of the gasses, and more particularly muriatic acid gas. Having been favoured some time ago by Mr. Davy with the Memoir, in which these speculations are announced, I have had it in my power to submit to examination the evidence on which they are supported, and to prosecute an experimental investigation to which this led. Some account of the results of these researches may not be unacceptable to your chemical readers; and to Mr. Davy, I am confident, I need offer no apology, for bringing under review a question of such interest, and at the same time of such a nature, that when surveyed under different aspects it may suggest very different conclusions.

and as connected with the relation of water to the constitution of some of the gasses.

The common phenomena, with regard to the production of oximuriatic acid, and its apparent analysis, admit of explanation equally on the established theory, and on that advanced by Mr. Davy. It is necessary therefore to take notice only of those, which are more peculiar; and, supposing your readers acquainted with Mr. Davy's memoir, I shall confine these observations to the facts, which he has stated as favourable to his opinion.

The common phenomena explicable on either theory.

Facts considered as most favourable by Mr. Davy,

When oximuriatic acid gas and hidrogen gas are mixed in nearly equal volumes, they act on each other, and are entirely

Composition of muriatic acid gas from the

oximuriatic acid and hidrogen, not decisive:

tirely converted into muriatic acid gas. This experiment Mr. Davy regards as synthetic, the oximuriatic acid and hidrogen forming he supposes the muriatic acid by their combination. There is obviously however another mode of accounting for the result. Regarding oximuriatic acid as a compound of muriatic acid and oxigen, the oxigen may be supposed to combine with the hidrogen and form water, while the muriatic acid remains in the elastic state. Mr. Davy has remarked indeed, that, when the gasses have been rendered as dry as possible, there is no sensible deposition of water; but no difficulty attends the supposition, that this may arise from the portion of water which is formed being retained in combination with the acid gas.

or the decomposition of sulphuretted hidrogen by oximuriatic acid gas:

Oximuriatic acid gas decomposes sulphuretted hidrogen, sulphur is deposited, muriatic acid gas is produced, and, when the gasses have been previously dried, the condensation Mr. Davy found is not equal to $\frac{1}{10}$. This decomposition, in conformity to his hypothesis, must be supposed owing to the hidrogen combining with the oximuriatic acid, and forming muriatic acid. According to the common theory the results must be ascribed to the oxigen of the oximuriatic acid combining with the hidrogen and forming water, which the muriatic acid holds combined with it; and the experiment presents nothing inconsistent with this explanation.

or that of ammonia by the same gas:

Ammonia, it is well known, is decomposed by oximuriatic acid. Mr. Davy has stated, that, in making this experiment, he has found, contrary to what has been asserted, that there is no sensible production of water; there is a condensation of nearly the whole acid and alkaline gasses, nitrogen gas remains, and dry muriate of ammonia is formed. The experiment therefore affords no evidence of the existence of oxigen in oximuriatic acid, but neither is it inconsistent with that hypothesis; for the water, that, in conformity to it, must be supposed to be formed, may exist in combination with either or both of the principles of the compound salt.

or the cases where oxigen is procured from oximuriatic acid:

Gay-Lussac and Thenard have proved, it is remarked by Mr. Davy, by a copious collection of instances, that, in the usual cases where oxigen is procured from oximuriatic acid,

acid, water is always present, and muriatic acid is formed: and as oximuriatic acid, he adds, is converted into muriatic acid gas by combining with hidrogen, it is scarcely possible to avoid the conclusion, that the oxigen is derived from the decomposition of water. Such facts however admit equally of explanation on the supposition, that the oxigen is derived from the decomposition of the oximuriatic acid, but that water is necessary to aid this decomposition by the affinity it exerts to the muriatic acid. They are not therefore of themselves more favourable to the one hypothesis than to the other.

When muriatic acid gas is acted on by metals, the acid disappears, and hidrogen gas is produced. In these cases Mr. Davy supposes, that the muriatic acid is decomposed, its oximuriatic acid combines with the metal, and its hidrogen is evolved, and accordingly the compounds formed are the same he remarks as those produced by the direct action of oximuriatic acid on the same metals. But this explanation rests on the previous assumption of the truth of the theory, and the phenomena are equally explained by supposing the hidrogen to be derived from the decomposition of the portion of water combined with the acid in its gaseous state, the oxigen of this water combining with the metal, and the oxide thus formed uniting with the real acid. It is true, that this production of hidrogen takes place even when the muriatic acid gas has been previously exposed to substances having a strong attraction to water, so as to bring it to its driest state; but the same explanation may still be given, for when the acid is in this state it can only be affirmed, that a portion of water has been abstracted from it, not that the whole has been withdrawn. As the water is removed from the acid gas, the affinity by which the remaining quantity is retained must progressively increase in strength, until it equal or exceed that of the substance attracting it; when of course its abstraction must cease. The phenomena then afford no decisive proof of Mr. Davy's hypothesis, for before the explanation given of them in conformity to that hypothesis can be received as exclusively just, it must be established by prior and independent evidence, either that muriatic acid does not contain combined water, or that it is a compound of oximuriatic acid and hidrogen.

or the action of metals on muriatic acid gas:

If

or the inability to extract oxigen from compounds formed by the action of oximuriatic acid gas on inflammables.

If the compounds formed by the action of oximuriatic acid gas on metals, or inflammables, contain oxigen, as they must on the common hypothesis of the constitution of that acid be supposed to do, indications of it may be discovered in the products of their decomposition, and thus evidence may be gained of the existence of oxigen in that acid. With the view of determining this Mr. Davy instituted several experiments. From the substance formed for example by the action of oximuriatic acid gas on tin, he endeavoured to procure oxide of tin by the addition of ammonia; and from that formed by the action of the same acid on phosphorus, he endeavoured by a similar method to obtain phosphoric acid. But in neither case was the experiment successful, the ammonia remaining in the combination, and forming products possessed of very singular properties, and in particular very difficult of decomposition. Still it may be remarked these experiments do not preclude the supposition, that oxigen may have existed in these combinations; for, as the compounds were not decomposed, it cannot be affirmed precisely what is their composition. They afford it is true no proof, that oxigen had been communicated from the oximuriatic acid; but neither do they give any proof, that this had not been the case. They may be compounds of the oxigenated metal or inflammable with muriatic acid and ammonia; and the difficulty of decomposing them, and abstracting the acid so as to discover this oxigenated base, may, in common with all the preceding facts, be ascribed to the peculiar relation of this acid to water, the absence of that portion of it necessary to the constitution of the acid gas preventing its disengagement from the base, precisely in the same manner that the same acid cannot be expelled from the dry muriate of potash or soda by a concrete acid, unless water be supplied.

The singular fact, that perfectly dry charcoal will not decompose oximuriatic acid gas, more in favour of his hypothesis:

There is one fact, which Mr. Davy has established, extremely singular; and which appears more conclusive against the hypothesis, that oximuriatic acid is a compound of muriatic acid and oxigen. If charcoal, freed from hidrogen and moisture by intense ignition in vacuo, be heated even to whiteness in oximuriatic acid gas, it produces no change in it. Now if oximuriatic acid be a compound of oxigen with muriatic

muriatic acid; and especially if, according to the common opinion, this oxigen is retained in combination with the acid by an affinity comparatively weak; charcoal might be supposed capable of abstracting it, at least with as much facility as hidrogen, which there is no reason to believe has a stronger attraction to oxigen. On the common hypothesis there appears no cause why it should not do so; and the fact seems favourable to the theory, that hidrogen causes the disappearance of oximuriatic acid, not by abstracting oxigen from it, but by combining with it, and converting it into muriatic acid. It is this experiment accordingly, Mr. Davy has added, which "led me to doubt of the existence of oxigen in that substance, which has been supposed to contain it above all others in a loose and active state."

but why does not the charcoal combine with this new acidifying principle?

I may first remark with regard to this singular fact, that it presents an anomaly just as great according to the new as according to the old hypothesis. Oximuriatic acid is considered in the former as a principle belonging to the same class as oxigen, which like it exerts powerful attractions to inflammable substances, and which is not inferior in energy of action. Why therefore should it not like oxigen combine with charcoal?

Probably because nothing is present to form the water essential to the constitution of the muriatic acid gas,

But farther it appears to me, that a solution may be given of this difficulty; that this apparent anomaly is connected with the preceding facts; and that the theory of it is to be derived from the peculiar relation of muriatic acid to water. Admitting that water combines intimately with this acid, and that it is essential to its transition to the elastic form, it is obvious, that, when charcoal is heated in oximuriatic acid gas, although it might attract the oxigen, and combine with it, there is no source whence the portion of water essential to the production of the muriatic acid gas can be supplied; the absence of this water therefore prevents the transition to the state of muriatic acid, and of course prevents the decomposition of the oximuriatic acid, and the consequent oxigenation of the charcoal. It is possible even that this may be aided by a similar necessity, though to a less extent, of the presence of water to the constitution of carbonic acid; as there are facts, which render it probable, that a portion of combined water exists in this

and perhaps also of the carbonic acid gas.

this gas, and is necessary to its transition to the elastic form. In treating charcoal in oximuriatic acid gas, all the circumstances are thus unfavourable to the oxigenation; as the products of this, the muriatic acid, and the carbonic acid, both require water to their constitution, while there is no source whence it can be supplied. There is another principle, which also probably operates in giving rise to this apparent anomaly. When a metal is placed in oximuriatic acid gas, its oxigenation is probably the result of that singular kind of chemical action named disposing affinity, being promoted, according to the language usually employed to express this, by the affinity of the muriatic acid to the metal in its oxidated state. The combination of hidrogen with the oxigen of oximuriatic acid is in conformity to the same law probably promoted by the attraction of muriatic acid to water. But this cause does not operate when charcoal is heated in oximuriatic acid gas, for there is no sensible, or at least no forcible attraction exerted between muriatic acid and the product of the oxigenation of charcoal, whether it be carbonic oxide or carbonic acid. Hence too charcoal is not acted on by liquid muriatic acid, while metals are oxidated and dissolved by it.

Want of what is called disposing affinity probably too operates in this case.

Of this phenomenon therefore, apparently so singular, an explanation, which I conceive is satisfactory, may be given, while it remains an anomaly on the opposite theory; for, on the assumption that oximuriatic acid is an acidifying principle having a strong attraction to inflammable bodies, no cause is assigned, why it should not combine with charcoal, as well as with hidrogen, or with metals.

This anomaly not so easily accounted for on Mr. Davy's hypothesis.

It will probably now be apparent, that all these facts are connected with the general question, whether water combines intimately with muriatic acid, and is essential to its constitution in the elastic state. On the assumption that this is true, they are capable of being explained by the common theory of the relation between muriatic and oximuriatic acid; on the denial of this proposition, the theory Mr. Davy has proposed affords another mode in which they are accounted for. But neither of these hypotheses (for as such only can they be regarded) is established by these facts

The question is therefore, whether water be essential to the constitution of muriatic acid gas.

to

to the exclusion of the other, and both, therefore, stand precisely on equal grounds.

Unity and simplicity of the common theory.

Comparing them for a moment under this point of view, the unity and simplicity of the common theory are much in its favour. There is some improbability in the hypothesis of two acidifying principles distinct from each other, but exerting similar chemical agencies. The explanations which this involves of the combinations of these principles with inflammables and metals, and of the relation of water to them, are extremely complicated; while on the more simple system opposed to this, nothing more is required than the assumption, if not proved, at least not improbable, of the existence of a portion of combined water in muriatic acid gas. And instead of any generalization, there is in the formation of the hypothesis, that oximuriatic acid is an acidifying principle, an evident adaptation to the phenomena peculiarly related to this question; since it does not appear, that this principle enters into the composition of any acid but the muriatic, or gives rise to acidity in combining with any substance but hidrogen.

Modes of deciding the question.

Instead of leaving the subject, however, on these general grounds, it were desirable to gain more conclusive evidence. There are two modes of proof, by which the question may be determined. Either it may be shown, that muriatic acid does, or does not contain combined water; or it may be proved, that oxigen does or does not exist in oximuriatic acid. It is only necessary, that the facts, on which the conclusion with regard to these propositions is to rest, should not be capable of being accounted for by the one hypothesis as well as by the other; and to attain this is extremely difficult from the facility with which both can be adapted to the phenomena.

Argument from water being necessary to the decomposition of a muriate by an acid inconclusive.

Thus no proof would appear at first view more decisive of the existence of combined water in muriatic acid gas, and the necessity of this to the transition of the acid to the elastic form, than that afforded by the fact established by the experiments both of Gay-Lussac and Thenard, and of Mr. Davy—that if a dry muriate, muriate of potash for example, be heated with a concrete acid, as the phosphoric or boracic, it is not decomposed, nor is any muriatic acid gas disen-

disenguged; but if a little water be added, very abundant vapours of the acid immediately arise. The obvious conclusion from this is, that water is essential to the constitution of the muriatic acid gas. Yet this is avoided by supposing in conformity to Mr. Davy's hypothesis, that dry muriate of potash is not a compound of muriatic acid and potash, but of oximuriatic acid and potassium; and farther supposing, that the water added is decomposed, its hidrogen combining with the oximuriatic acid, and converting it into muriatic acid, and its oxigen changing the potassium into potash.

Indirect argument for the existence of water in the driest muriatic acid gas.

There are some indirect considerations, however, from which the existence of combined water in muriatic acid gas may be inferred, or which at least give probability to such a supposition. Thus it is obvious from the facility with which this acid when in the gaseous state is condensed by water, and from the large quantity of it that is absorbed, that an affinity of considerable strength exists between them. In consequence of this affinity the acid in its gaseous state must retain a portion of water combined with it; and from the law of chemical attraction so well established, that quantity adds to its force, it follows, that this water will be retained in combination with a strength of attraction progressively increasing as it is abstracted; and that a portion, therefore, must probably remain, in the driest state to which muriatic acid gas can be brought.

The probability of this strengthened by the analogy of carbonic acid gas.

The supposition, that muriatic acid contains combined water, and that this is essential to its gaseous state, is rendered more probable by this influence of the combination of water being established with regard to other elastic fluids, where there is not the same ambiguity. Carbonic acid cannot be easily expelled by heat, or even by the action of an acid, from native carbonate of barytes; while it is expelled from the artificial carbonate with facility; though the only difference between the carbonate in these forms is, that in the one it contains a portion of water, and in the other it appears to contain none. And accordingly, if water be applied to the native carbonate, it favours its decomposition, and the expulsion of the carbonic acid. In this case is clearly established the influence of water with regard

regard to carbonic acid, and its transition to the elastic state; and this cannot be explained by any hypothesis similar to that, by which the analogous influence of water with regard to muriatic acid is attempted to be explained. It renders more probable, therefore, the explanation in conformity to the common theory with regard to this acid inferred from similar facts. It may be added, that the affinity of muriatic acid to water being evidently stronger than that of carbonic acid, the former in its gaseous state may be presumed to retain a larger quantity combined with it than the latter.

Proof of the existence of oxigen in oximuriatic acid not easily obtained.

The other mode of proof, whence this question may be determined, is that relating to the existence of oxigen in oximuriatic acid. The facts from which this has usually been inferred, either by the apparent formation or decomposition of the acid, it has already been remarked are ambiguous, as they admit equally of explanation on Mr. Davy's hypothesis. The object is to obtain proof not liable to this ambiguity, and one train of investigation occurred to me, whence I imagined such proof might be obtained.

Decomposition of the acid by charcoal with the addition of water gives imperfect or ambiguous results.

The cause which I have assigned for charcoal not being oxigenated by oximuriatic acid is, that the portion of water necessary to the constitution of muriatic acid gas is not afforded. If water, therefore, were supplied, the oxigenation ought to take place. There is some reason to believe that this is the result, but from the state of aggregation of the charcoal, it is not easy to conduct the experiment, so as that it shall not be either imperfect or ambiguous.

Argument from oximuriatic acid and carburetted hidrogen forming muriatic and carbonic acids.

But there is another form under which it may be made, and with regard to which indeed the facts are already partly ascertained. If oximuriatic acid gas, and any of the varieties of what are named carburetted hidrogen gasses be mingled together, it is known from the experiments of Cruickshank, that a mutual action is exerted, either immediately if the electric spark is taken in the mixture, or more slowly if they are allowed to remain in their mixed state; and in either case, if the due proportions are observed, they are entirely converted into muriatic and carbonic acids. The explanation of this according to the common theory is obvious; the carbon and hidrogen of the inflammable gas receiving oxigen from the oximuriatic acid, carbonic acid and water

water are formed, and this water must be supposed to be retained in combination with the muriatic acid, into which the oximuriatic is converted. But it does not admit equally of explanation on Mr. Davy's hypothesis. The oximuriatic acid may be supposed indeed to combine with the hidrogen of the carburetted hidrogen to form muriatic acid. But whence is the oxigen derived, which converts the carbon into carbonic acid? The result, in strict conformity to the hypothesis, ought to be the conversion of the oximuriatic acid into muriatic acid, and the deposition of the carbon in a state analagous to charcoal. This is indeed the result when some of these gasses are acted on by a certain quantity of oximuriatic acid; but when the quantity is larger, the product is uniformly carbonic acid.

Only apparent source of ambiguity

There appears to me to be only one source of ambiguity attending this experiment, and that probably a very trivial one. The inflammable gasses, which have been regarded as binary compounds of carbon and hidrogen, are probably rather ternary compounds of carbon, hidrogen, and oxigen; and on this supposition it may be said, that, while the oximuriatic acid combines with the hidrogen, and forms muriatic acid, the carbon and oxigen of the inflammable gas remain in union, and form carbonic acid. This explanation however implies the existence of a much larger quantity of oxigen, than there are any grounds for supposing does enter into the composition of these gasses. It besides fails to account for the large quantity of oximuriatic acid that is required. Little more than one measure of oximuriatic acid gas to one measure of hidrogen is required for mutual saturation, so that the whole is converted into muriatic acid gas. But not less than four measures of oximuriatic acid gas to one measure of carburetted hidrogen are necessary to produce the full change into carbonic acid; and if less be employed, there is either a deposition of charcoal, or a formation of carbonic oxide. Now this quantity cannot be required merely for the saturation of the hidrogen contained in the carburetted hidrogen, and what other agency can it be supposed to exert, by which it produces the conversion of the carbon

answered.

bon into carbonic acid, than that of communicating oxigen?

The experiment however it occurred to me might be made in a manner free even from this ambiguity. Cruickshank observed, that, when carbonic oxide is mixed with oximuriatic acid gas, on taking the electric spark in the mixture there is no detonation produced, nor any sensible change. This must be ascribed, on the common hypothesis, to the same cause as the nonoxigenation of charcoal in oximuriatic acid gas—the absence of that portion of water, which is essential to the constitution of muriatic acid gas. But he observed, that, if the two gasses were mixed together, a mutual action is slowly exerted, and after twenty four hours they are converted into carbonic and muriatic acids. Now this is a result different from what the view of the subject I have given would lead to expect, and apparently too in opposition to the result obtained when the action of the electric spark has been employed; for there is no obvious cause why the oxigenation of the carbonic oxide should be effected in the one case, more than in the other. This difference it appeared probable depended on the agency of water. When the electric spark is taken in the mixture of the gasses, even though they may not have been previously dried, the quantity of water within the sphere of agency of the spark is so inconsiderable, that the action is not favoured. But in the slow action the whole water diffused through them can operate successively, and therefore with more effect, so as to admit of at least a partial oxigenation, and if the gasses have been passed through water into the bottle in which they are allowed to remain mixed, (the manner probably in which the experiment was performed by Cruickshank) the film of water adhering to the internal surface may render the action complete. To ascertain if this supposition were just, I made the experiment in the following manner.

Carbonic oxide and oximuriatic acid gasses not changed by the electric spark,

though they are by standing together.

This difference owing probably to the presence of water.

Two measures of carbonic oxide gas, and two measures and a half of oximuriatic acid, each having been previously exposed to muriate of lime, were mixed together. The mixture could not be passed through quicksilver without the quicksilver being acted on by the acid, so that a portion

Experiment to ascertain this. The two gasses made as dry as possible stood together 24 hours without

being decomposed:

tion of it adhered to the sides of the jar, and continuing to act on the acid might counteract any action on the carbonic oxide. To avoid this, the mixed gasses were introduced into a caoutchouc bottle connected by a tube and stop-cock with a jar containing dry mercury. On turning the stop-cock, and suddenly compressing the elastic bottle, the gas descended into the jar, and depressed the mercury without acting upon it, this action being prevented still more effectually by a little pure carbonic oxide having been previously introduced into the jar. The stop-cock being shut, and the jar being also closed at the bottom, the mixed gasses were allowed to stand 24 hours. At the end of this time the green tinge of the oximuriatic acid gas was perfectly apparent; the stop-cock being turned under quicksilver, a very slight diminution of volume, not exceeding $\frac{1}{10}$ of the whole mixture was indicated by the entrance of a portion of quicksilver equivalent to this. On transferring to water, there was no immediate absorption, but the oximuriatic acid gas was gradually condensed. The residual gas was then transmitted through lime water in a long narrow tube, it communicated a milkiness to the surface, so slight as to be barely perceptible; this was removed on agitation, and did not appear on a second or third transmission. The residual air was found to be carbonic oxide, both by the test of oximuriatic acid aided by water, and by being converted by combustion into carbonic acid. In this experiment then the carbonic oxide had scarcely been acted on by the oximuriatic acid, and the very minute quantity of carbonic acid that appeared to be formed must be regarded as owing its origin to the impracticability of abstracting water entirely from the gasses, or to the presence of a small proportion of hidrogen in the carbonic oxide.

but they were decomposed in this time, if a little water were admitted, or they were merely passed through water.

I now mixed carbonic oxide and oximuriatic acid in the same proportions as in the preceding experiment, and allowed a little water to remain in the bottle. On opening it under water after 24 hours, the carbonic oxide was found to be converted into carbonic acid, and the oximuriatic acid into muriatic acid, so that after agitation with lime water to absorb the former, scarcely any sensible quantity of elastic fluid remained. The same result was obtained when the gasses were merely

merely transmitted through water into the bottle, the small quantity of water diffused through them, and adhering to the glass, being sufficient to promote their mutual action.

The rationale of both experiments is obvious. The circumstance of there being no mutual action in the first experiment must be ascribed to the absence of that water essential to the constitution of muriatic acid. In the second experiment this is supplied; hence the transfer of the oxigen from the oximuriatic acid, and the consequent formation of muriatic and carbonic acids. But they do not appear to admit of an equally satisfactory explanation on Mr. Davy's hypothesis. For the peculiarity that carbonic oxide suffers no change in the first experiment no cause can be assigned, but that oximuriatic acid has no tendency to combine with carbonic oxide or its base; and this is an evident anomaly on the assumption, that oximuriatic acid is a principle exerting like oxigen powerful affinities to inflammable bodies. To account for the agency of the water in the second experiment it must be supposed, that it suffers decomposition, affording hidrogen to the oximuriatic acid, and oxigen to the carbonic oxide. But there are no just grounds, whence this can be inferred. Water is not decomposed by oximuriatic acid gas, or by carbonic oxide gas; there is no reason to conclude, that its decomposition can be effected by their action when they are presented to it merely in a mixed state; and the more obvious operation may be regarded as the real one, that it acts by its affinity to muriatic acid.

This readily accounted for by the common theory,

but not by Mr. Davy's.

But even this source of ambiguity I supposed might be obviated by making the experiment under another form. I mixed together one measure of carbonic oxide gas, one measure of hidrogen gas, and two measures and a half of oximuriatic acid gas, each previously having been exposed to muriate of lime, and introduced them into a jar in the same manner as in the first experiment. The green tinge of the oximuriatic acid gas gradually disappeared, and after two hours exposure to clear day light was not apparent. On opening the jar after 24 hours under quicksilver, a diminution of volume, less as the gasses had been completely dried, was indicated

Experiment tending farther to obviate the ambiguity.

Hidrogen mixed with the dry gasses promoted their decomposition,

dicated. On transferring it into water a farther diminution took place, the water filling rather more than half the jar. On passing the gas through lime water it communicated to it an abundant milkiness, and this repeatedly. A small portion of air remained, which on approaching a lighted taper to the orifice of the tube exhibited a weak flame.

though there was no source of oxigen to form the carbonic acid, on Mr. Davy's hypothesis.

In this experiment then the greater part of the carbonic oxide had been converted into carbonic acid. On repeating it with rather a larger proportion of oximuriatic acid, three measures and a half to one measure of carbonic oxide and one of hidrogen, the changes were similar, but even more perfect, the residual air being scarcely, if at all, inflammable. This result appears to be conclusive in proof of the existence of oxigen in oximuriatic acid, and to be free from the ambiguity which attends the experiment where water is employed to favour the action of the gasses. Hydrogen only is supplied, and how is its agency to be explained? On the one theory it may be conceived to form water by attracting oxigen from a portion of the oximuriatic acid, and this, favouring its transition into muriatic acid, promotes its farther decomposition, so as to afford oxigen likewise to the carbonic oxide, and convert it into carbonic acid. But in the opposite theory, although it may be assumed that the hidrogen will convert the oximuriatic into muriatic acid, what is to produce the oxigenation of the carbonic oxide?

A mixture of carbonic oxide and oximuriatic acid gas exposed to the electric spark for 15 min. The carbonic oxide remained unchanged.

Cruickshank had found, that no explosion is produced by the electric spark in a mixture of carbonic oxide and oximuriatic acid gasses. To confirm the preceding results, I repeated this experiment with a due attention to the object I had in view; one measure of dry carbonic oxide, and two measures of oximuriatic gas being subjected to the action of the electric spark over quicksilver for fifteen minutes. No detonation was produced, nor any sensible effect, farther than a diminution of volume to the extent of half a measure from the action of the quicksilver on the oximuriatic acid. On removing the excess of oximuriatic acid gas, by water, the residual air occupied almost exactly the volume of one measure; when transmitted through lime water it did not render it milky; it burned with

with the blue flame of carbonic oxide, and was converted into carbonic acid. In this experiment then the carbonic oxide had evidently sustained no change.

A similar mixture, with the addition of hidrogen, exploded, and the gasses decomposed.

I now put into the tube one measure of dry carbonic oxide gas, one measure of dry hidrogen gas, and three measures of oximuriatic acid gas. On transmitting the electric spark there was an instant explosion with the production of a whitish vapour, and diminution of volume. On removing the tube into water the vapour was condensed, a slight tinge of oximuriatic acid remained, but this was soon removed with farther diminution of volume by the action of the water. The residual air occupied scarcely one measure, and by the test of lime water was found to be carbonic acid with a small portion of atmospheric air. A similar result without any remaining excess of oximuriatic acid was obtained on using rather a larger proportion of hydrogen.

This confirms the preceding explanation.

Here the ultimate results are the same as in the experiments on the slow mutual action of these gasses, and the same explanation is to be given. There is the same difficulty too in accounting for them by Mr. Davy's hypothesis; for although the hidrogen might convert a portion of the oximuriatic acid into muriatic acid, there is no source whence the oxigen, which has converted the carbonic oxide into carbonic acid, can have been obtained; and the results according to that hypothesis ought to be merely the disappearance of the hidrogen, and the formation of a portion of muriatic acid, while the carbonic oxide with about half of the oximuriatic acid remained unchanged.

Objection to the conclusions drawn,

There is only one mode, in which the force of this conclusion drawn from the results of the experiments in which hidrogen is employed, whether in the slow action, or in the detonation by electricity, can be attempted to be obviated. It is that of assuming, that the carbonic oxide is not converted into carbonic acid previous to the admission of water, that it forms with the hidrogen and oximuriatic acid a ternary compound which decomposes the water, and attracts its oxigen and hidrogen, so that the carbonic oxide becomes carbonic acid, and the oximuriatic, muriatic acid. But this is a supposition, which, were it advanced, would be altogether gratuitous, and had recourse to merely to avoid a

gratuitous,

difficulty; for there is no evidence of the formation of such a compound from any phenomena, which attend the experiment. This objection too was in a great measure obviated by introducing into the electric fluid, previous to the admission of water, a slip of dry litmus paper: it instantly received a bright red tinge, a proof of the production of acid.

and in great measure obviated.

In making these experiments I was assisted by my friend Mr. Ellis; they were often repeated, and every attention was paid to ensure accuracy. The carbonic oxide was obtained first from dry chalk and zinc filings; and lest this should contain a little hidrogen, which might be a source of errour, more particularly in the experiment of the slow action of the gasses, it was afterward prepared from native carbonate of barytes and iron filings. The oximuriatic acid was prepared from muriatic acid and oximuriate of potash. The influence of light in promoting the action of the oximuriatic acid on the inflammable gasses was strictly attended to, the mixture in the different experiments being always exposed for the same length of time to the clear light of day.

The experiments often carefully repeated with similar results.

From these experiments there appears to be sufficient evidence of the existence of oxigen in oximuriatic acid; and I may remark, that carbonic oxide is peculiarly calculated to afford this evidence without that ambiguity, which attends the action of oximuriatic acid on other inflammable substances, as in these the substance which is formed enters into combination with the muriatic acid, and the nature of these compounds becomes the subject of hypothesis. In the action of oximuriatic acid on potassium, for example, a substance is formed, which according to the common theory is a compound of oxide of potassium (potash) with muriatic acid; but which, according to the opposite hypothesis, is a compound of oximuriatic acid and potassium: and it is not easy to submit it to any experiment, from which it may be determined which of these views is just. But in the case of carbonic oxide, the muriatic acid, having no great attraction to the product, does not combine with it; its nature therfore is discovered, and the obvious production of it from the carbonic oxide proves, that oxigen must have been communicated, and that it could be so only from the oximuriatic acid.

Hence we have sufficient evidence of the existence of oxigen in oximuriatic acid.

In

In reflecting on other cases of chemical action exerted by oximuriatic acid on inflammable substances, there appeared to be one, in which nearly the same advantage might be expected. From its action on sulphur a compound is formed, which, in conformity to Mr. Davy's hypothesis, is composed of oximuriatic acid and sulphur; but which, according to the other, consists of sulphur in some state of oxidation, and muriatic acid, free or nearly so from water. But when sulphuretted hidrogen is acted on, a different result may be expected: the hidrogen receiving oxigen from the oximuriatic acid will form water, which favouring the production of muriatic acid, oxigen will likewise be communicated to the sulphur, and convert it into sulphurous or sulphuric acid; to either of which the muriatic acid exerting no strong attraction, their production will be apparent. When oximuriatic acid gas and sulphuretted hidrogen gas are mixed together over water, it is known, that a rapid diminution of volume takes place, and sulphur is deposited. Mr. Davy has stated the results more minutely, excluding the agency of water. The two gasses, previously dried, were mixed in equal volumes; the condensation was not $\frac{1}{10}$, sulphur which seemed to contain a little oximuriatic acid was formed on the sides of the vessel, no vapour was deposited, and the residual gas contained $\frac{19}{20}$ of muriatic acid, the remainder being inflammable.

Farther evidence from the action of oximuriatic acid gas on sulphur,

and on sulphuretted hidrogen.

This result appeared to me to arise probably from the proportions of the two gasses, the quantity of oximuriatic acid not being sufficient to afford oxigen to convert both the hidrogen into water, and the sulphur into sulphuric acid; and the hidrogen suffering the oxigenation most rapidly, so that the sulphur is deposited without being acted on. If a sufficiently large proportion were employed, it seems to follow from the preceding reasoning, that the oxigenation of the sulphur to a certain extent at least ought to take place, the formation of water from the oxigenation of the hidrogen favouring the transition of the oximuriatic to muriatic acid. Proceeding on this principle I obtained the following results from experiments varied with regard to the proportions. When one measure of oximuriatic acid gas was mixed over water with two measures of sulphuretted hidrogen,

Different results from different proportions of the gasses.

1 part oximuriatic gas and 2 sulphuretted hidrogen.

hidrogen, copious dense fumes were produced with a deposition of sulphur on the sides of the jar, and on the surface of the water rendering it turbid. When these proportions were reversed, there was also the production of a dense vapour and a deposit of sulphur; but the water, which was at first rendered turbid, soon became transparent as it rose in the vessel, the film of sulphur disappeared, and the entire gas nearly was condensed. When oximuriatic acid gas was allowed to escape from a retort into sulphuretted hidrogen gas in a jar, a very dense vapour was produced as each bubble of gas broke from the surface of the water, sulphur was collected on the surface, and the water was quite turbid. When the experiment was reversed, the oximuriatic acid gas being in the jar, and the sulphuretted hidrogen allowed to escape into it, a whitish vapour was also produced as each bubble of gas broke, but much less dense and less in quantity, and there was scarcely any deposition of sulphur; obviously owing to this, that the sulphuretted hidrogen from its inferior specific gravity rapidly diffused itself through the mass of oximuriatic acid gas, which thus acting upon it in large quantity oxigenated both the hidrogen and the sulphur, while in the other mode the reverse of this happened*.

1 oxim. acid, 2 sulph. hidrogen.

Oxim. acid gas received in sulph. hidr.

Sulph. hidr. received in oxim. acid gas.

Experiments with these gasses repeated in different forms.

To exclude the agency of water, one measure of sulphuretted hidrogen was admitted into an exhausted tube, and afterward three measures of oximuriatic acid gas. A whitish vapour immediately spread through the tube, but it was not very dense, nor was there any deposition of sulphur;

Singular phenomenon.

* A singular appearance presented itself in these experiments. As each bubble of oximuriatic acid gas broke in the sulphuretted hidrogen, there was an explosion with a vivid flash of light, though no evident combustion attends the direct mixture of the two gasses, and there was no similar appearance when the sulphuretted hidrogen broke in successive bubbles in the oximuriatic acid gas. The difference may be owing principally to the influence of the specific gravity of the gasses as above explained, the oximuriatic acid, when introduced into the sulphuretted hidrogen, from its greater density not diffusing itself, and therefore acting in a more concentrated state; partly also perhaps to a slight change produced on oximuriatic acid by transmission through water.

and

and the air in a few minutes regained its transparency. On introducing the gasses in equal volumes into the tube a very dense vapour was produced, with a copious deposition of sulphur. The experiment was repeated in another form. Oximuriatic acid gas was introduced into a glass globe with a stopcock, and expanded by heat so as to produce a partial exhaustion. On admitting sulphuretted hidrogen in small successive portions, a very slight vapour was at first produced, and it soon disappeared; but on the subsequent admissions it became more and more dense. Part of the residual air, after having regained its transparency, was expelled from the globe by heat; and on admitting a fresh portion of sulphuretted hidrogen, there was a copious deposition of sulphur on the sides of the globe. This however after some hours had in a great measure disappeared. On turning the stopcock under water, there appeared to be little diminution of volume, a small portion of water only entering the narrow tube of the stopcock; but when this was diffused through the globe by agitation, on again opening it water rushed in so as nearly to fill it. It had therefore been filled with muriatic acid gas which the water instantly condensed; and as there was little deposition of sulphur it may be concluded, that the greater part of it had been converted into sulphuric, or partly perhaps into sulphurous acid. The water was acid and gave a precipitate with muriate of barytes. The results of these experiments thus sufficiently accord with the supposition, that, when sulphuretted hidrogen is acted on by oximuriatic acid, the sulphur receives oxigen when the due proportion of acid is employed.

General observations.

All the facts connected with the subject of the preceding researches appear in the present state of the investigation to lead to the adoption of one of two general systems, each of which involves several subordinate propositions necessarily connected. In the one—that proposed by Mr. Davy, oximuriatic acid is regarded as a simple substance, and muriatic acid as a compound of this substance with hidrogen. In the other—oximuriatic acid is held to be a compound of muriatic acid and oxigen; the composition of muriatic acid itself is unknown, but it is supposed to have a peculiar relation

tion

tion to water in consequence of which they combine intimately, this combined water powerfully modifying the affinities the acid exerts. In the former system oximuriatic acid is supposed to form an extensive series of compounds with inflammable and metallic bases; muriatic acid forms another order of compounds with these bases in an oxidated state; and the former of these compounds are converted into the latter, when water is communicated to them, by the water being decomposed, its oxigen being transferred to the base, and its hidrogen to the oximuriatic acid. In the latter hypothesis the facts connected with these combinations are explained by supposing the one order of compounds to be composed of muriatic acid free from water combined with inflammables and metals in an oxidated state: the difficulty of decomposition, the character by which they are distinguished from the common muriates must be ascribed to the absence of the water essential to the existence of muriatic acid in an insulated form; and the changes they suffer from the communication of water must be supposed to arise from the changes it produces in the chemical powers of the acid. In the latter hypothesis the only difficulty which appears to me of importance is that arising from this peculiar relation between muriatic acid and water. It undoubtedly presents an anomaly in chemical theory; for although there is no difficulty in supposing from the evident affinity which exists between these bodies, that the acid in its elastic form may hold a portion of water combined with it, it is not obvious by what agency this water should be so essential to its existence in this form, or should have so important an influence on its chemical powers. All the other phenomena however appear to admit of explanation on this hypothesis, and if the conclusions I have ventured to draw from the preceding experiments be just, it is supported by more direct evidence. The peculiarity with regard to water must therefore be elucidated by farther investigation, and it presents an interesting subject of research.

Only anomaly on the old theory,

which presents an interesting subject of research.

I have the honour to be,

Sir, yours, &c.

J. MURRAY.

Questions

IX.

Questions on the Nature of Water. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

WILL you permit a young Tyro in chemistry, to solicit information through the medium of your Journal, upon a few observations of some of your celebrated chemists, which have excited his attention, and unsettled his ideas.

In the last edition of Mr. Parkes's Chemical Catechism, p. 106, is the following note. "Some recent speculations "of Mr. Davy seem to involve the conclusion, that wa- "ter is not a compound body, but the ponderable base "both of *oxigen* and *hidrogen* gasses, assuming either of "these forms according to its electrical states. See "Murray's Chemistry 2d ed. vol. II, 136; or Mr. Davy's "paper in the Phil. Trans. for 1808."—Now there is not in Dalton's New Chemical Philosophy, or in the 6th edition of Dr. Henry's Elements of Experimental Chemistry (both very recently published) any mention of any such an idea;—the latter considers *hidrogen* as a metallic body, in a state of *opposite electricity* to *oxigen*; for, like other inflammable substances, it is naturally in a state of *positive* electricity, and he distinctly says, that the *two gasses* have *different bases, oxigen* and *hidrogen:* and that water is a compound, not of the two gasses, but of oxigen and hidrogen.

Questions on the nature of water.

In the 1st vol. of Dr. Henry's Elements, p. 223, last edition, he says, "When friction is applied to the glass "cylinder of an electrical machine, the electric fluid flows "to it from surrounding bodies, and thence passes to the "prime conductor, in which it exists in a greater than "natural quantity. All then that is effected, by the "action of the machine, is a disturbance of the natural "quantity of electricity in bodies, or a transfer of it from "some to others, in consequence of which, while the latter "acquire a *redundance*, the former become proportionally "*deficient* in this quantity of electricity." And he adds, in a note, "This theory appears to me to have several ad-

"vantages

Questions on the nature of water.

" vantages over the hypothesis, which supposes *two different* " *kinds of electricity*, capable by combination of neutra- " lizing each other. The only modification, I would propose, " is a slight change of terms; and the adoption of those " suggested by Mr. Cuthbertson, viz. *condensed* and *rare-* " *fied*, instead of *positive* and *negative*, electricity"—and yet Dr. Henry, not only in the instance before quoted, but in the subsequent parts of his valuable work, upon this subject, observes, throughout, the distinction of *positive* and *negative* electricity*.

In page 173 of Mr. Parkes's book is a note, in which he insinuates, that, as Mr. Davy has discovered that what is termed *oxigen* is the principle of *alkalescence*, as well as of *acidity*, the term *oxigen* is improper, and that *hidrogen* would be a more unobjectionable term.

If any of your correspondents will favour me with remarks upon these points, I shall feel myself under great obligations to them.

I am, Sir,

Your constant reader,

A. B.

Cheltenham,
January 5, 1811.

REMARKS.

The hypothesis, that water constitues the base of oxigen and hidrogen, is by no means new. It was thus that Dr. Priestley, and others who maintained the doctrine of phlogiston, accounted for the appearance of water in Mr. Cavendish's experiment of the combustion of hidrogen with oxigen. Some have even considered water as the ponderable base of all the gasses. The opinion, however, that water is a compound of oxigen and hidrogen, has long been generally admitted; and though Mr. Davy observed, in the paper alluded to, that his experiments at that time seemed in some degree favourable to a modified phlogistic theory, yet a constant reader must have seen, that he considered this opinion by no means supported by his subsequent experiments. C.

* When I wrote the foregoing, I had not seen the article in your last Journal on the electric effects of friction.

X. *Remarks*

X.

Remarks on Mr. La Place's *Theory of Capillary Action. In a Letter from* Thomas Knight, *Esq.*

To Mr. NICHOLSON.

SIR,

Objection to La Place.

AMONG the objections that may be made to Mr. La Place's theory of capillary action, there is one, that I should be glad to take notice of, through the medium of your valuable publication.

The errour alluded to (as well as many others) I have indeed pointed out, in an "Examination of Mr. La Place's Theory," published about a year ago: but it appears, from the present number of the Monthly Review, that what I very clearly expressed has been totally misrepresented. *I hope* it has been *misunderstood* *.

Action of a meniscus and of a fluid with a plane surface considered separately: but the same mode of reasoning applicable to a mass of any form;

It is convenient, in Mr. La Place's view of the subject, to consider, *separately*, the action of a concave meniscus, and that of a fluid mass with a plane surface, on a slender column of fluid.

This author shows, in a satisfactory and ingenious manner, that, by the action of the meniscus, the column is urged upwards. Now, the same mode of reasoning that is applicable to a meniscus, must also be applicable to a mass of any other form. That very *same* reasoning ought, therefore, to have been employed in determining the action of the mass with a plane surface: and thus it would have been found, that by *this also* the column is urged *upwards*.

though Mr. L. endeavours to show the contrary.

Instead of this, Mr. La Place, by a process of quite a different kind (which the Monthly Reviewer esteems vastly simple and ingenious), attempts to show, that this mass with a plane surface urges the column *downwards*.

His errour shown.

The falsehood of which may be proved in a moment; for, if we consider any point in the mass, it is easy to see that it will urge the column neither way, or will urge it upwards: and this, by Mr. La Place's own reasoning which he had used when treating of the meniscus.

* The author passes over every material argument in the examination; and has the prudence not to defend Mr. La Place's general theory.

I affirmed,

I affirmed therefore before, and I affirm with the same "*boldness*" again, that, by employing reasonings *totally opposite* in cases *exactly similar*, Mr. La Place and Mr. Haüy did virtually contradict in one passage what they had said in another.

It is asserted by the Monthly Reviewer, in the course of his *pleasant* observations, that, "If they had been guided "by conceptions as vague and uncertain as those of their "examiner, they would to this time have been wandering "in errour."

His results true, from a compensation of errours.

I will show briefly, that, if they had proceeded according to those conceptions, they would have obtained the *same results*, though in a manner somewhat different; consistently, and without arriving at truth by a compensation of errours. Instead of making the mass with a plane surface urge the column *downwards*, with a force K, this letter would have represented its action *upwards*, which added to the action of the concave meniscus, in the same direction, would have given the whole force, with which the column is urged *upwards*, $= K + \frac{H}{b}$*. So that, from this method, there would have arisen ultimately, by the principle of a canal, the equation

$$K + \frac{H}{2}\left(\frac{1}{R} + \frac{1}{R'}\right) - gz = K + \frac{H}{2}\left(\frac{1}{b} + \frac{1}{b'}\right)$$

which, by transposition, is the same as the one now given by La Place, (in p. 19 of his work) and must necessarily lead to the same results.

To conclude—If Mr. La Place had found his fundamental equation in the manner I have pointed out; and, instead of making the whole column support itself, had *truly* shown how it was held up by the tube: there would not, I think, have been any thing to object to his theory†.

I am, Sir,

Your most obedient servant,

Papcastle, Dec. 8th, 1810. THO. KNIGHT.

* Mr. La Place makes it to be urged *downwards* by a force $= K - \frac{H}{b}$. where I use his notation.

† But *then* it would have been a *different* theory.

XI,

XI.

On Muriatic and Oximuriatic Acid, in Answer to Justus. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

YOUR correspondent who signs himself *Justus* in your last number seems to possess considerable acquaintance with the subjects of his animadversion; but in regard to oximuriatic and muriatic acid at page 72, his data are defective, and the principles of his calculations are to me altogether unintelligible. If he will explain the manner of his calculation, I shall probably on some future occasion reply to his different inquiries and observations.

On muriatic and oximuriatic acid.

I remain, yours,

J. DALTON.

Manchester, Jan. 20th, 1811.

XII.

An Analysis of Fluor-Spar. By THOMAS THOMSON, M. D. F. R. S. E*.

THE mineral called fluor-spar has been long known, and valued on account of its beauty, and the ease with which it can be turned on the lathe into various ornaments and useful utensils. It occurs chiefly in veins, and very frequently accompanies lead ore. Some of its properties have been described more than a century ago, as, for example, its phosphorescing when heated, and its corroding glass when mixed with sulphuric or nitric acid. But it is not forty years since its composition was discovered by Scheele, who demonstrated, that it is composed of lime, and a peculiar acid, called fluoric. Chemists now distinguish it by the name of fluate of lime.

Fluor spar, or fluate of lime.

Hitherto, no chemical analysis of this salt has been published, except a very incorrect one by Kirwan and Gren, which has been ascribed to Scheele, though I cannot find it in any of his dissertations on fluor-spar. By that analysis, it is made to contain 27 *per cent* of water, a proportion very inconsistent with the properties of native fluate of lime,

No accurate analysis of it yet published.

* From the Transactions of the Wernerian Society.

which

which, when strongly heated in a wind furnace, loses at an average only $\frac{1}{600}$th part of its weight. The obvious inaccuracy of the analysis given by the authors just mentioned induced me, to make a set of experiments on it last summer (1807). I selected the purest, transparent, colourless crystal, which I found by repeated trials to be very nearly pure fluate of lime. When reduced to a fine powder, and digested in nitric acid, I found in the acid only a little lime, owing doubtless to the partial decomposition of the fluate, and minute traces of iron and lead: these two metals I detected, by evaporating the nitric solution to dryness, heating it to redness, and then dissolving the residue in muriatic acid. The colour of the solution showed the presence of iron, and a few needleform crystals of muriate of lead were deposited after the solution had stood for some days. The fluate, which I used, had been dug out of a lead mine in Northumberland, and small crystals of sulphuret of lead were here and there to be seen in it. Probably some one of these had escaped my attention, and, by being mixed with the fluate which I used, occasioned the appearance of the lead, which, however, did not amount to $\frac{1}{3000}$th part of the salt, and therefore could not occasion any sensible errour in the subsequent analysis.

Experiments on it.

Digested in nitric acid.

I first tried to decompose the fluate of lime, by fusing it with twice its weight of carbonate of potash in a platinum crucible. Only a small portion of the fluate was decomposed. I was, therefore, obliged to repeat the fusions very often, washing off the alkali after each operation by means of water, and then dissolving the carbonate of lime formed, in muriatic acid. Fatigued with the tediousness of this method, and despairing of an accurate result from the great number of successive solutions, I abandoned it altogether, and adopted the following method, much more expeditious and equally precise.

Fused repeatedly with carbonate of potash.

From a mass of fluor spar, which I had ascertained to contain no sensible portion of foreign matter, I separated 100 grains, which I reduced to powder, and digested for some hours in a platinum crucible, with rather more than an ounce of pure concentrated sulphuric acid. The mixture was then evaporated to dryness, and the crucible exposed

100 grains digested in sulphuric acid,

posed for an hour to a strong heat, in a wind furnace. To ensure the complete decomposition of the fluor, the mass was reduced to powder, and treated a second time in the same manner with another ounce of sulphuric acid. The residue, which was white with a slight shade of red, proved on examination, to be pure sulphate of lime. It weighed 156·6 grains.

gave 156·6 of sulphate of lime,

Now, it has been ascertained by the most careful experiments, that sulphate of lime, thus violently heated, contains 43 per cent of lime. Of consequence, the whole quantity of lime in 156·6 grains of sulphate is 67·34 grains. This is obviously the whole lime contained in 100 grains of fluor spar; and since that mineral contains no sensible portion of water, the remainder of the 100 must be fluoric acid. Hence fluate of lime is composed of,

consequently contained 67·34 grs of lime,

Lime	67.34	and 32·66 fluoric acid.
Fluoric acid	32·66	
	100·00	

This result differs very materially from the analysis alluded to in the beginning of this paper, which makes the amount of the acid in the same weight of spar, only 10 grains, and the lime 57 grains; the remaining 27 grains being considered as water. But from the care with which my experiment was conducted, I flatter myself, that the result which it exhibits is very near the truth*.

SCIENTIFIC NEWS

Mr. George Singer's course of Lectures on Electricity and Electrochemistry will commence on the 7th of February, at the Scientific Institution, Prince's Street, Cavendish Square. They embrace not only all that has formerly been done, but the most recent discoveries of Mr. Davy, and Mr. De Luc's analysis of the Voltaic pile, an account of which has appeared in the present and two preceding volumes of our Journal.

History and practice of electricity.

St. Thomas's and Guy's Hospitals.

The Spring Courses of Lectures at these adjoining Hospitals will commence the beginning of February, as usual.

Medical Lectures.

TO CORRESPONDENTS.

Mr. Moore's and Mr. Gregor's communications were too late for the present number, but will appear in our next; as will Mrs. Ibbetson's on the corollas of flowers.

* Since the preceding paper was read, I have seen an analysis of fluor spar by Klaproth, in the 4th volume of his Beitrage, conducted in a very different manner from mine, but leading to almost the very same result. He found fluate of lime composed of 67¾ lime, and 32¼ acid — quantities which coincide with mine, within less than 1 per cent. — Nov 1809.

The result agrees with Klaproth.

METEOROLOGICAL JOURNAL,

For JANUARY, 1811,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

DEC. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
28	37°	35°	39°	29°	30·08		Snow	Fair
29	34	34·5	38.5	28	·36		Hail	Ditto
30	32	32	34	27	·50		Fair	Ditto
31	31·5	32	33	26	·50		Snow	Cloudy
JAN.								
1	30·5	32	32	26	·26		Ditto	Ditto
2	29	30	30	24	·04		Ditto	Snow
3	27	29	30	21·5	29·71		Ditto	Ditto
4	26	28·5	32	23	·90		Cloudy[b]	Cloudy[b]
5	25	28	30	23·5	·87		Ditto[c]	Ditto[c]
6	25	26	30	24	·80		Fair[c]	Fair[c]
7	27·5	29·5	29·5	24	·88		Ditto	Ditto
8	27	27·5	30	25	·84		Ditto	Ditto
9	29·5	31·5	32	23·5	·81		Snow	Cloudy
10	25	35	35	35	·93		Foggy	Rain[a]
11	41	37	42·5	34	·68	·130	Fair	Fair
12	40	44	45	34·5	·66		Rain	Rain
13	38	44	44·5	40	·65	·060	Ditto	Ditto
14	45	44·5	47	41	·63	·120	Ditto	Cloudy
15	45	37	45	32	·62	·015	Fair	Fair
16	36	38	40	37	·86		Ditto	Cloudy
17	46	47	50	34	·77	·045	Cloudy	Ditto
18	41	37	42	32	·77		Rain	Fair
19	35	38	41	32	30·32	·020	Ditto	Ditto[a]
20	35	34·5	36	31	·40		Fair	Ditto
21	35	39	40	32	·05		Rain[a]	Foggy
22	35	34	37	33	·21		Foggy	Ditto
23	37	40	41·5	34	·28		Ditto†	Ditto
24	37	38	41·5	33	·42	·010	Fair	Fair
25	36·5	35	37·5	28	·50		Ditto	Cloudy
26	31	41	42	37	·26		Ditto	Ditto
27	40	35	41	27	29·61	·030	Ditto	Ditto

·430 Inch. since last Journ. not including Snow.

[a] Intervening fogs. [b] Stormy. [c] Boisterous. [d] Immeasurable, † and small rain.

Erratum last Journal.—Dec. 25, for 29·31 read 29·00.

Nicholson's Philos. Journal, Vol. XXVIII. P. V. p. 161

Motion of Rockets.

Fig. 1.

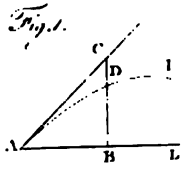
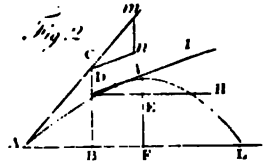


Fig. 2



Structure of the Petals of Flowers

Fig. 3

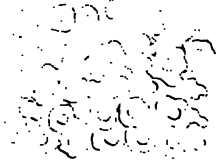


Fig. 5

Fig. 4

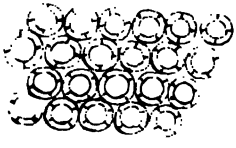


Fig. 6

Fig. 7

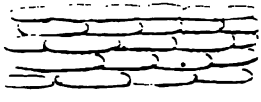


Fig. 10

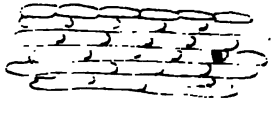


Fig. 8

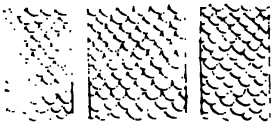


Fig. 11

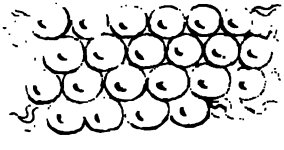


Fig. 9

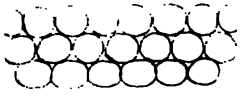
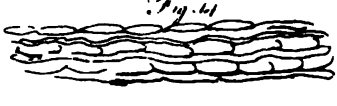


Fig. 13

Fig. 12

Fig. 14



A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

MARCH, 1811.

ARTICLE I.

On the Motion of Rockets both in Nonresisting and Resisting Mediums. *By* W. Moore, *Esq.*

(Continued from vol. XXVII., p. 285.)

To Mr. NICHOLSON.

Sir,

IN my last paper on Rockets I ascribed the invention of those now used by the English to Colonel Congreve:—My assertion rests in the fact that this gentleman has been rewarded by government for the discovery; and my own conviction, that, anterior to such reward being granted, Invention of military rockets.

a proper enquiry could not but have been instituted concerning his pretensions.

I herewith send you a further continuation of my essay on the motion, &c. of these machines, which I hope will be found worthy your acceptance; and am,

Sir,

Your much obliged humble servant,

W. MOORE.

Royal Academy, Woolwich,
Jan. 1811.

Prop. II.

To determine the path of a rocket near the Earth's surface, neglecting the resistance of the atmosphere.

Path of a rocket in a nonresisting medium.

If during the time the rocket was on fire, the weight of the whole mass remained constant, the path of the rocket would, by mechanics, be a straight line: but this being not the case on account of the continual wasting of the matter which feeds the flame of the rocket, the accelerative force of the body will be different at every instant; and therefore, since the accelerative force of gravity (as we will suppose) is constant to the height which rockets generally ascend, the rout of the rocket will consequently be a curvilinear one.

Now, although gravity acts to impel the rocket from a straight course, yet it does not hinder the same from arriving at

at a line parallel to the direction in which that force is exerted in the *same* time that it would have done by the single action of its own impelling force. Therefore, in order to determine the curve which the rocket will describe under the true circumstance of its motion, let us suppose that if gravity had not acted, the rocket would have arrived at C, Pl. V. fig. 1, in the line of its first direction A C in the time t. Then in this case we shall have (Prop. 1.) the space described in the time t, namely $A C = x = \frac{b c}{2 a m} \times \left(t^2 + \frac{c t^3}{3 a m} + \frac{c^2 t^4}{6 a^2 m^2} + \frac{c^3 t^5}{10 a^3 m^3} + \frac{c^4 t^6}{15 a^4 m^4} + \&c. \right)$

Or multiplying by $\frac{2 a m}{b c}$ ($=$ suppose Q) and calling the coefficients of the several terms of the series A, B, C, &c.; it will be $Q x = t^2 + A t^3 + B t^4 + C t^5 + D t^6 +$ &c.; which reverted into a series of x is $t = \overline{Q x}^{\frac{1}{2}} - \frac{A}{2} \cdot Q x + \frac{5 A^2 - 4 B}{8} \cdot \overline{Q x}^{\frac{3}{2}} + \frac{3 A B - 2 A^3 - C}{2} Q^2 x^2 +$ &c. $= Q^{\frac{1}{2}} \times \left(x^{\frac{1}{2}} - \frac{A}{2} \cdot Q^{\frac{1}{2}} x + \frac{5 A^2 - 4 B}{8} \cdot Q x^{\frac{3}{2}} + \frac{3 A B - 2 A^3 - C}{2} \cdot Q^{\frac{1}{2}} x^2 + \&c. \right)$; the time of describing the distance x along A C from the commencement of motion.

Now C D (y) being the distance descended by gravity in the same time; we therefore get $\frac{1}{2} \surd y$ (omitting the $\frac{1}{2}$) for the time of the rocket's describing C D by the force of gravity: and consequently $\frac{1}{2} \surd y = Q^{\frac{1}{2}} \times \left(x^{\frac{1}{2}} - \frac{A}{2} \cdot Q^{\frac{1}{2}} x + \frac{5 A^2 - 4 B}{8} Q x^{\frac{3}{2}} + \&c. \right)$

Hence, knowing the equation which subsists between A C and C D the track which the rocket describes may be drawn; for it will only be necessary to give some value to x in order to determine the corresponding value of y, and to lay off this upon C D drawn perpendicular to A B,

and thus finding several points of the curve, the curve itself may be described.

We have here supposed gravity to act in parallel lines, which is not strictly true; but the distance to which a rocket ranges on the earth's surface being so very small compared with its circumference, the errour arising from the contrary supposition will not in any material degree affect our conclusions.

Prop. III.

To find the velocity of the rocket in the curve at any given instant.

Velocity of a rocket at any given time.

In the preceding diagram let A C $= x$, and A D $= z$ being the space described by the rocket in the time t: Then calling the velocity at C $(= b \times \text{hyp. log.} \frac{a\,m}{a\,m - c\,t}$ (Prop. 1.) V; the velocity at D, in the curve, will be expressed generally by $\frac{\dot{z}\,V}{\dot{x}}$, following from the laws for the resolution of motion. Now by the laws of falling bodies in vacuo C D $= g\,t^2$: and putting k and l for the natural sine and co-sine (to rad. 1.) of the angle C A B of projection; we shall have A B $= l\,x$, C B $= k\,x$, and D B (the ordinate of the curve) $= k\,x - g\,t^2$. Therefore $\dot{z} = \left((k\,\dot{x} - 2\,g\,t\,\dot{t})^2 + l^2\,\dot{x}^2\right)^{\frac{1}{2}}$ and v $(= \frac{\dot{z}\,V}{\dot{x}}) = \left(\frac{l^2\,\dot{x}^2 + (k\,\dot{x} - 2\,g\,t\,\dot{t})^2}{\dot{x}}\right)^{\frac{1}{2}} \times V$. Again by the theory of variable motions $\dot{x} = V\,\dot{t}$. Consequently $v = \left(\frac{l^2\,V^2\,\dot{t}^2 + (k\,V\,\dot{t} - 2\,g\,t\,\dot{t})^2}{V\,\dot{t}}\right)^{\frac{1}{2}} \times V = \left(V^2\,l^2 + (k\,V - 2\,g\,t^2)\right)^{\frac{1}{2}} = \left(l^2\,b^2 \times (\text{hyp. log.} \frac{a\,m}{a\,m - c\,t}\right)^2 + \left(k\,b \times \text{hyp. log.} \frac{a\,m}{a\,m - c\,t} - 2\,g\,t^2\right)\Big)^{\frac{1}{2}}$ the velocity of the rocket at D; which wants no correction: because when $z = o$, $t = o$, and the whole vanishes: therefore

therefore $v = \left(l^2 b^2 \times \left(\text{hyp. log. } \frac{a m}{a m - c t}\right)^2 + (k b \times \text{hyp. log. } \frac{a m}{a m - c t} - 2 g t)^2.\right)^{\frac{1}{2}}$

When the angle of projection is 90°, $l = 0$, and $k = 1$: therefore v in this case will be $b \times$ hyp. log. $\frac{a m}{a m - c t} - 2 g t$; as determined in Prop. 1: and when $k = 0$, or the action of gravity is 0, the velocity of the rocket in its rectilinear path is $b \times$ hyp. log. $\frac{a m}{a m - c t}$ which agrees with what has already been observed.

Prop. IV.

To find the horizontal range of the rocket, having the angle of elevation of the engine, and the time the rocket is on fire given.

Horizontal range of a rocket.

Let D, fig. 2. be the place of the rocket when all the wild-fire it contains is just exhausted; and C m and C n the measures of the velocities of the rocket in the directions A C and D I, the latter of which is a tangent to the curve at D: then by trig. sin. $\angle$ C n m ($=$ n C B $=$ I D B) $=$ $\frac{\text{C } m}{\text{C } n} \times$ sin. $\angle$ C m n $= \frac{\text{C } m}{\text{C } n} \times$ co-sin, of the angle of elevation of the engine $= \frac{\text{Vel. at C}}{\text{Vel. at D}} \times$ co-sin. of the $\angle$ C A B. Whence calling the velocities at C and D, V and v (computed from the 3 Prop.) we have sin. $\angle$ I D B $= \frac{\text{V}}{v} \times$ co-sin. $\angle$ C A B. Since, then, we have found the $\angle$ I D B, it will be easy to determine that part of the range denoted by B L. For the curve from D being a parabola D H $= \frac{s u v^2}{g}$, and V E $= \frac{s^2 v^2}{4 g}$ (from the laws of projectiles in vacuo); where s and u represent the sin. and co-sin. of the $\angle$ I D H $=$ $\angle$ I D B $- 90°$; consequently V F $=$ V E $+$ E F $=$ V E $+$ D B $= \frac{s^2 v^2}{4 g} + k x - g t^2$; whereof x is given by the first proposition.

Again,

Again, by the nature of the parabola V E : V F : : E H^2 : F L^2 $= \frac{u^2 v^2}{g} \times \left(\frac{s^2 v^2}{4g} + kx - gt^2\right)$; and F L $= \frac{uv}{4} \times \left(\frac{s^2 v}{4g} + kx - gt^2.\right)^{\frac{1}{2}}$ Whence A L the entire range (= F L + B F + A B) $= \frac{uv}{4}\left(\frac{s^2 v^2}{4g} + kx - gt^2\right)^{\frac{1}{2}} + \frac{suv^2}{2g} + lx$ which was required.

For an example in numbers: suppose the engine from whence the rocket is thrown to make an angle with the horizon = 45°: and let all other things remain as in the first proposition. Then v, the velocity of the rocket in the curve at the end of its burning $= \left(l^2 b^2 \times (\text{hyp. log. } \frac{m}{m-c})^2 \times \left(kb + \text{hyp. log. } \frac{m}{m-c} - 6g\right)^2\right)^{\frac{1}{2}} = \sqrt{4479024 + 4080400} = \sqrt{8559424} = 2925{\cdot}6$; and sine angle I D B $= \frac{V}{v} \times$ co-sin. $\angle$ C A B $= \frac{2993}{2925{\cdot}6} \times$ co-sin. $\angle$ C A B $= 134° 6' 38''$. Whence angle I D H $= 44° 6' 38''$; the nat. sin. and co-sin. of which are ·6960172 and ·7180251 $= s$ and u respectively: and the values of the letters in the above expression for the range collectively are as under.

s	=	·696
u	=	·718
v	=	2925·6
k	=	·7071
l	=	·7071
x	=	4159·6
g	=	16
t	=	3

Whence the range itself is readily found = 273116·29 feet or 51·72657 miles.

On the Motion, &c. of Rockets in a resisting Medium.

Prop. V.

Motion of a rocket in a resisting medium.

The strength or first force of the gas from the inflamed composition of a rocket being given, as also the weight of the quantity of composition the rocket contains, together with the

the time of its burning, and the weight and dimensions of the rocket; to find the height to which it will rise in the atmosphere if thrown perpendicularly, and also the velocity acquired at the end of that time; the law of resistance being supposed to vary directly as the square of the velocity; and the laminæ of the composition to fire uniformly and to burn parallel to the rocket's base.

Put w = weight of the case of the rocket and head
c = weight of the whole quantity of matter with which it is filled
a = time in which the same is consuming itself uniformly
n = 230 ozs.
s = 1000
d = diameter of the rocket's base
x = P D the space the rocket describes in the time t
v = the acquired velocity in that time
R = the resistance of the air to the rocket when moving with a velocity of b feet per second

Then $b^2 : v^2 :: R : \frac{R v^2}{b^2}$ the resistance at D; and consequently $s n e d^2 - (m - \frac{c t}{a}) - \frac{R v^2}{b^2}$ (see Prop. 1) will be the motive force of the rocket at D in this case; and $\frac{(s n e d^2 b^2 - R v^2) a}{(a m - c t) b^2} - 1$ the accelerative force. Therefore $\dot{v} = 2 g f t' = \frac{(s n e d^2 b^2 - R v^2) 2 g a t'}{(a m - c t) b^2} - 2 g t'$; or putting $2 a g \times s n e d\ b^2 = h$, $2 a g R = k$, $a m b^2 = l$ and $c b^2 = p$, we shall have $\dot{v} = \frac{h t' - k v^2 t'}{l - p t} - 2 g t'$; and $l \dot{v} - p t \dot{v} = h t' - k v^2 t' - 2 g l t' + 2 g p t t'$; and further, putting $h - 2 g l = q$ to render the expression as simple as possible, it will be $l \dot{v} - p t \dot{v} - q t' + k v^2 t' + 2 g p t t' = r$; where r may be determined in terms of t as follows;

Assume $v = A t + B t^2 + C t^3 + D t^4 + E t^5 +$ &c.; then making $t' = 1$; we have $\dot{v} = A + 2 B t + 3 C t^2 +$

4 D

$4\,D\,t^3 + 5\,E\,t^4 +$ &c.: and substituting these in the given equation it becomes as below; namely,

$$\begin{aligned}
l\,\dot{v} &= l\,A + 2\,l\,B\,t + 3\,l\,C\,t^2 + 4\,l\,D\,t^3 + 5\,l\,E\,t^4 + \text{&c.}\\
-p\,t\,\dot{v} &= -p\,A\,t - 2p\,B\,t^2 - 3p\,C\,t^3 - 4p\,D\,t^4 - \text{&c.}\\
-q\,\dot{t} &= -q \quad - - - - - - - - - - - - - - - - -\\
k\,v^2\,\dot{t} &= \qquad\qquad -k\,A^2\,t^2 - 2k\,A\,B\,t^3 - (2\,A\,C + B^2) \times k\,t^4 - \text{&c.}\\
2gpt &= 2gpt \quad - - - - - - - - - - - - - - - -
\end{aligned}$$

By adding which we evidently obtain

$$0 = 0 \qquad 0 \qquad 0 \qquad 0 \qquad 0$$

Whence, equating the homologous terms, to find the quantities A, B, C, &c. they become

$$A = \frac{q}{l}$$

$$B = \frac{pq - 2gpl}{2l^2}$$

$$C = \frac{p^2q - 2gp^2l + kq^2}{3l^3}$$

$$D = \frac{p^3q - 2qp^3l + 2kpq^2 - 2gkpql}{4l^4}$$

$$E = \frac{35kp^2q^2 - 52gkp^2ql + 8k^2q^2 + 12g^2kl^2p^2 + 12p^4q - 24lp^4q}{5l^5}$$

&c. &c.

Therefore the fluent required is $v = \frac{q}{l}t + \frac{pq - 2glp}{2l^2}t^2 + \frac{p^2q - 2glp^2 + kq^2}{3l^3}t^3 + \frac{p^3q - 2lp^3q + 2kpq^2 - 2gklpq}{4l^4}t^4 + \frac{35kp^2q^2 - 52gkp^2ql + 8k^2q^2 + 12g^2kl^2p^2 + 12p^4q - 24lp^4q}{5l^5}t^5 +$ &c. And hence the space x is readily found $= \frac{q}{2l}t^2 + \frac{pq - 2glp}{6l^3}t^3 + \frac{p^2q - 2glp^2 + kq^2}{12l^4}t^4 + \frac{p^3q - 2lp^3q + 2kpq^2}{20l^5} - \frac{2gklpq}{}t^5 +$ &c.; where, making $t = a$ in both expressions, the values of v and x as required by the proposition will be determined.

SCHOLIUM.

Scholium.

In this solution, the resistance of the air to the rocket's motion is supposed to vary directly as the square of the velocity; an hypothesis which experiments disprove when applied to military projectiles with cannon balls: but it is to be apprehended, that in the motion of rockets the deviation from this law is scarcely to be regarded, since, what takes place in the flight of shot and shells to violate it, is in a great measure obviated in the rockets, by the extreme heat of the flame that rushes from them; this rarifying the ambient air promotes the motion of the particles striking the head of the rocket towards its hinder parts; and since it is only the immediate motions of such particles backwards that can cause the law to obtain (for it would obtain correctly, if, after the impact of the particles they had no power to impel others lying before them, but either glided off from the surface struck, or had their force annihilated by it at the moment of striking) it is to be expected that the conclusions here brought out which are grounded on this law of resistance, will be found to agree pretty correctly with the results determined from experiment. Scholium.

But if they should not, let then the law of resistance be as the nth power of the velocity; and the method of solution will remain precisely the same as before. For it is only the fourth equation in the preceding process, namely, $k v^n t' =$ &c. that will vary or become affected by any deviation from the law we have assumed; and therefore when this shall have been settled by experiment (the only way in which it ever can be settled), and the absolute resistance determined in any one case of velocity, and the real strength of the rocket composition ascertained; then, and *not till then*, shall we be able to offer any *unerring* rules to the military practitioner.

II.

The beautiful Tint of Flowers acquired by the same Means that paint the Rainbow. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Dissection of the corolla of flowers.

AS I mean to dedicate the present year to the consideration of the interior of plants in general, I cannot better begin it, than by giving to the public the dissection of the *corolla of flowers*, a subject in itself so very curious, that I scarcely know any in botany (where utility is not concerned) that more merits the attention of the inquisitive. By taking the petal of each different sort of flower, and splitting it, drawing off the upper and under skins, and leaving only the pabulum or middle part to be examined singly, and then magnifying each skin alternately, the most exact result is gained; which I shall now lay before you.

Petals of flowers derive their beauty from the aqueous particles in their pabulum.

The petals of flowers owe none of their beauty to the colour that paints them, which is, when drawn off, *dull* and *dead*: nor do they owe their brilliant tints to the *skin* that covers them: but the greatest part of their loveliness is derived from the *bubbles of water*, that compose their pabulum. Receiving the sun's rays they are enlivened and brightened by the reflection and refraction, from those *drops of water*, and from that spot of light seen in every bubble of water, and striking to the focus underneath them; by which means the whole flower would at times be a blaze of light, had not nature, to soften it, covered the petal with an upper and under skin, which curtails their diamondlike rays, and leaves them only a lightness and beauty unequalled. Uncovered they would resemble the rainbow formed by the same means (though coloured by the division of light): but, shaded as they are with a gauzelike matter, they acquire a more chastened tint, and are equally delightful, without being oppressive to the sight. It must be remembered, that I treat only of flowers which have *regular* petals,

petals, and touch not on the cryptogamiæ, or any that possess not those direct parts.

Taking the corolla of flowers in this strict sense, they may be divided into five different sorts: the thin petal, such as the *rosa*; the moist petal as found in the *hyacinthus*; the velvet denoted by the *viola tricolor*, and sweet scabious; the thick petal by the *magnolia* and *lilium*; and lastly, the everlasting, by the xeranthemum. Five kinds of corolla.

The petals of most flowers differ from leaves in many respects, but particularly in one essential point; in leaves the coloured part is within, and the upper skin (even of the darkest leaves) is white; but in flowers, the coloured part is without, and the *white* is within; for almost every flower (the orange excepted) have their pabulum or interior white. Petals differ from leaves in their colour being external.

Most thin petals (which I shall first describe), have, like the rose, when its upper surface is peeled off and examined, an extremely thin skin, in which are tiny bladders of pink liquid, woven in a sort of gauselike texture. But when this is taken off, it displays a pabulum of white or rather water bubbles, to which nature, (to lessen the brightness,) has added two circular white lines, which give a tenderness to the pink impossible to describe. The common violet is formed in the same manner; but the darker colour and thicker skin lessen the sparkling of the water. Thin petals. The rose. The common violet.

To prove that bubbles of water are the cause of the beauty that flowers generally transmit, either in vivid flashes, or tender tints, to the retina; take the dullest colour that was ever painted, and, filling a small glass bubble with water, let the rays of the sun fall through it on the colour, and it will become the brightest and most beautiful imaginable; and exactly resemble the tint of flowers. It was this discovery, that made me more than five years ago dedicate a whole summer to dissecting the petals of flowers, which I suspected to be beautified and enlivened by water. I then proved my ideas were just, that their tints were formed by colours transmitted through water, and like clouds and the rainbow owed their perfection to this cause; but I shall recur to this subject at the conclusion of my letter. Experiment to show how much colours are brightened by the sun's rays transmitted through water.

The

Moist petal. The second or moist petal is so filled with water, that it only excites our astonishment how such a thin gauselike matter can contain such a quantity of liquor; and yet the flower reposes on the hand without wetting it. No person can examine it, and not see that it owes its sparkling appearance to the water it contains; for when we look at it in the sun, it almost dazzles the eyes. When I said that flowers owed the beauty of their tints to the bubbles, I did not mean all their charms: there are other arts by which they shine, and well deserve that encomium our blessed Lord bestowed: "not Solomon in all his glory was arrayed like one of these."

Velvet petal. The velvet petal I was long discovering the formation of. I saw it differed from every other, yet could not understand how, as it was evidently in the upper surface; for when this was taken off, its pabulum resembled that of the rose: but putting it a little sideways in the solar microscope, I soon

Its structure. found out the secret. A vessel extremely diminutive is carried up and down in a scallop, as at *a a*, Pl. V, fig. 10, and each of the upper points divided. Why this produces such extreme softness, it would be very curious to inquire. The fact however is certain; it is by this means we make velvet, chenille, &c.; it is this that causes such extreme softness in feathers in general; in miniature painting it is this makes a stroke so much softer than a dot; and is it not the down on the cheek of beauty, which gives so exquisite a softness, that paint must directly destroy? Is is that the ray of light, instead of being repelled or absorbed, is reflected down the scallop, and therefore declines by degrees? It is curious that our method of making velvet should be precisely the same by which these flowers are formed, so that the studying of nature would have taught us that art. But how many secrets may she not teach, if she was pursued with avidity! To proceed, this scallop is found in every flower, which has this velvetty appearance, though in some it is deeper, in others less deep, and therefore not so soft: still it is always a scallop.

Thick petal. The next petal I am to explain is the thick. It is composed of a thick but double surface skin; the pabulum having fourteen or sixteen layers of extremely diminutive round

round bubbles, placed in rows. If the sun falls on the magnolia, when the surface is taken off, it will shine like so many little stars. Sometimes the upper skin is in round bladders, sometimes long; but the former produces the most brilliant appearance. These thick flowers always decay the soonest, as the little water they contain is quickly evaporated.

Everlasting petal.

The everlasting in its name declares its nature. Unlike all the rest, it retains a part of its beauty, and owes it to its being coloured by a *powder*, instead of *water*. I found little else in the bubbles, which are long, and have a very thin pabulum. But to show, that some of our flowers may owe their beauty to other contrivances beside water, I shall name a plant common in the fields, which greatly adorns them, and is known by the vulgar name of butterflower, the ranunculus. The petals appear to be varnished; but this arises from a powder, which exactly resembles calcined magnesia, and lies between the pabulum and upper skin. This is easily proved to be the cause of its shining appearance; for in the claw there is no white powder, nor is there any varnish. The powder in the everlasting flowers is probably formed in the same manner, and of the same nature, which will account for their not decaying: though the ranunculus does not last, because the pabulum under the white powder is full of water, which is not the case with the everlastings.

Manner in which the petal is supported & strengthened.

I shall next describe the means by which these thin transparent flimsy bodies are sustained and strengthened, so as to bear much pressure, and to preserve their elegant shapes often in spite of wind and rain. In describing the formation of leaves in a former letter, I mentioned two sorts of vessels belonging to them, the *nourishing* and *spiral vessels*: they equally belong to the corolla, and enter it up the claw of the petals, the latter forming always the middle stroke, while the nourishing vessels adorn each side: see Pl. VI, fig. 1. It is a beautiful provision of nature, that those muscles that serve to turn, to open and shut the flower, in short, to manage it in every way, should also be its means of strength; for the spiral vessels being in their cases very stiff and strong, when compared to a petal, certainly serve this double

double purpose: and it is curious, that in some petals (the thick for example) where the thickness supplies the place of support, the spiral wires are often found without a case. It is in watching these little minute circumstances we come at the secrets of nature; for when we place her in unnatural situations, to understand her intentions, we only draw conclusions to deceive ourselves. No one would for a moment doubt, that these spiral wires governed the petals, who would watch them for hours as I have done for days together. I have seen a flower suddenly contract its muscles, even before the rest of the corolla could obey the motion; seen a sudden cold air draw them to breaking; or seen them expand with a hot sun, or on being breathed upon, till they could no longer obey any impulse. I know not any better means to show their muscles, than exposing flowers either to a very hot fire or an icehouse; both extremes act equally. The moment they contract with violence, so that each muscle is seen through its cover; then relaxing at once, they hang like mere wet rag, having neither form nor strength, and decay directly. On examining a dying flower, it will be found, that it is the muscles which draw the petal together, and not the skin. I know no better flowers for showing the power of the spiral wire, than the deadelphian tribe. The art with which the banner of all peaflowers is contrived has always excited my admiration. How admirable the management! and when, as in the visia faba, the case would not be strong enough to distend it, there is formed a double and treble layer of vessels, which form a strengthening piece for the whole, and which the case surrounds. Observe the strength of the keel, when the finger is passed over it; how admirably it is contrived to withstand both wind and rain, and to form a hollow in which the stamens and pistil are sheltered from every danger. Who can examine the wings of the lathyrus, and not be struck with astonishment at the art it betrays? how much thinking would it cost a mechanic, to invent such a contrivance! and how poor, how coarse, would his execution have been! Can any person see without admiration the petals of the iris susiana, and the elegance of those folds? But I should never have done if I expatiated on all the forms and shapes I have so often contemplated with delight, and dissected for the sake of admiring

Muscles of flowers.

miring the contrivance of the whole: it is only to be conceived when seen. But to my dear countrywomen in particular I would recommend such employment: I am most anxious to interest and excite to study those of my own sex, who are still ignorant how much pleasure is attached to the developement of the human mind. These contemplations are not puerile, as many may think; they speak to the heart.

Formation of the rainbow resembles the colouring of flowers.

Let me now turn to the rainbow, and endeavour to show how exact the resemblance is. It would be superfluous here to enter into a detail of the manner in which the rainbow is formed by the refraction and reflection of the solar rays by drops of water; and since we know this to be the case, what should prevent flowers, formed of little else from receiving part of their hues by this means, while the colour given them is increased and embellished by the water it is seen through, and the sparkling light, which passes into most petals and adds to their beauty? To try the effect I got a quantity of extremely small glass bubbles of water blown for me, and placed them as in a petal—in rows. Although infinitely larger, yet they appeared a petal extremely magnified. I then covered them with a gauze, painted to represent the flower, and truly did it imitate the sort of brightness and brilliancy it was intended to represent.

Jacobean lily.

In the jacobean lily the surface is formed of an extremely thin skin of red liquid, but the pabulum is composed of round bubbles of yellow water, which together appear orange; but what causes that fiery bright spark, which animates the whole? the prismatic scarlet, which proceeds from that point of light the circular ball always gives, and which sparkling, has puzzled so many botanists? This is easy to be seen when the upper surface is taken off; for it then really dazzles the eyes. What causes the orange tint in the lily of that colour, or the crown-imperial? Its first skin is red, and its next green. I appeal to any painter to say, whether these will make orange? and yet the flower is truly so. What causes that exquisite tint of green in the andromeda pulverulenta, for the upper skin and pabulum are all white? probably here it is reflection, which it often is, at the bottom of a flower, or the deepening of a bud.

Lily and crown-imperial.

The

The bud alters not its colour, but its situation is changed, and much will depend on this. How many times have I looked for a tint in a flower, and found nothing in the least to resemble it? I was once extremely fond of painting, and sought in flowers for colours their beauty tempted me to think I should find. But how mistaken I was! I accused my own awkwardness; but, after many trials, was assured, that the deception was in the flower, which owed little of its beauty to the liquid that inflated it, on the contrary I was then convinced I must seek for the cause elsewhere, for in no one instance did the liquid of the flower serve even to stain the paper of the same colour as the flower; and when used, some chemical aid, some process to brighten and preserve, must be resorted to; for it is always dull and poor, so that no one could guess its origin; a proof, that the flowers owed not their beauty to the liquid colour, but to something that aided and enlivened it. And drying flowers! what things are they when done? they are not only bad in themselves, but of the most fatal consequence to botany, where flowers are consulted; for they change their colours, get hairy, and undergo many more alterations, which deceive, and cause the most unpleasant mistakes.

Paints not to be obtained from the juices of flowers.

Changes in flowers when dried.

As to the scent in flowers, it is very easy to trace their secretions, being in bladders, and always divided from the other juices (as in leaves). They are generally fixed between the upper skin and pabulum; though in some few flowers (as in the geranium) they are to be found in the scallop of the petal. The scent is in every flower an oil, and must be extremely strong, when such diminutive bladders can so perfume the air; if we could gain it pure, it would not want any chemical aid to strengthen it. It was those bladders that used to be taken for the largest pores in flowers.

Smell of flowers from an oil contained in bladders,

which have been mistaken for large pores.

I have now given a short sketch of corollas in general, I shall finish therefore by a few observations on the pores observed in flowers; and endeavour to show the extreme difference in botany between the two terms perspiration and evaporation. If I had published this letter two years ago, which I was on the point of doing, I should have said, as all the botanists do, that there were two sorts of

Difference between perspiration and evaporation.

Two sorts of

pores

pores in flowers. But I have now too deeply studied the part to be longer deceived. There are certainly none, either large or small, above or below the petal. Take the petal of a rose, and, drawing off the upper surface, divesting it of as much pabulum as possible, lay it on your finger, the under part uppermost; with a clean soft rag rub the part as long as you see any mark; and then place it on the glass of the microscope. If, on being magnified, it shows any black pieces, rub it again, and they will all disappear; and you will find its pinholes were merely the impression of the apertures in the pabulum, which showed through a skin of such extreme fineness, that it can be compared only to gold beaters skin; and that the surface of the petals is formed of this, and extremely thin pink coloured bladders, in which no pores are to be found, that are visible even with the solar microscope. Du Hamel thought he had found this sort of skin on the inside of the rind of trees and all plants: I before proved his conjecture true in this respect; which put an end to the possibility of branches and stems perspiring; but I had no idea I should find this skin in flowers. I have now in my sliders thirteen sorts of this skin, which keeps well, taken from such different orders and classes of flowers, that I must believe it is universal in all. Perhaps, but for this skin, these thin and flimsy flowers would never be able to bear any heat, and the water in them would directly evaporate. It is very astonishing to consider how it can be preserved in such delicate shapes. That there are airholes I do not in the least doubt, though they cannot be perceived; for we have proof, that vapour constantly escapes: but certainly no water, for a fluid requires a much larger aperture. Therefore in flowers perspiration and their imbibing the dew drops are both mistakes. I have no doubt the hairs carry water, and the dew, to the interior of the petal; but, that no water can pass through the skin, which I have just described, I have the most positive proof.

pores supposed in flowers, but there are none.

Rose leaf covered with a thin skin destitute of pores,

found also on the inside of the rind of trees and plants,

and apparently on all flowers.

But it has airholes.

Flowers give out no oxigen, but yield a great deal of other air, of so bad a quality that no candle can burn in it, and which does not diminish nitrous acid. It is however extremely moist: and in the various trials I have made, to

Flowers yield no oxigen, but an azotic air containing much moisture,

endeavour to ascertain the quantity of moisture formed in this vapour, which plants constantly yield, I have always been foiled, from the quickness of the evaporation. In weighing leaves, I fold them in blotting paper, first ascertaining its weight: but though the leaves have often lost near one thirtieth of their weight in two hours, yet the paper has scarce gained one third of that thirtieth: which, considering the different weight of air and water, is nothing, and certainly less infinitely than the moisture in that vapour.

Plants yield vapour only.

That plants yield vapour only, and that the moisture is very difficult to be separated from the air, is easily proved, by the method in which it is generally received. Put a plant fresh out of the ground, first wiping of the watery cryptogamiæ that may grow on it, and place it under a bell glass, and in an hour or two it will cover the interior with dew: but leave an opening in any part not so big as a sixpence, and no moisture will be found. Why is this? because in the first place, the air being pressed by the quantity of oxigen that pours from the plant, is by this pressure forced to give out its moisture; and the fluid, being detached from the air, is attracted by the glass, to which it has great affinity. But when the oxigen vapour has the power to escape, not being pressed, it retains its moisture, and both leave the glass in the same condition in which they quitted the pores. So I found it in weighing the leaves, which I have tried in every manner possible: the blotting paper was not pressure sufficient to detach the moisture, and the paper absorbed but little, though the leaves had evidently lost so much vapour.

Plants do not per-pire.

In former letters I endeavoured to show, that the watery cryptogamiæ and the vapour just mentioned were taken for the perspiration of plants: every trial, every year's study, convince me still more of this truth: *that plants do not perspire.* That they yield a quantity of vapour, I never doubted—but who, that has ever dissected or studied plants, can confound the terms of *perspiration* and *evaporation* together, in botany at least? Perspiration is the issuing of water from pores formed for the purpose. It was said by all the advocates of perspiration, to be accelerated by heat, retarded by cold, and ascertained by

by measurement: whereas evaporation is air and water mixed, issuing from pores so very diminutive, that no eye, even aided by the solar microscope, can see them. It can neither be accelerated, nor retarded; nor can it be ascertained, (since it disappears as fast as it is collected for the purpose). I before showed what the watery cryptogamiæ were, by drawings of many plants of the sort; nourished by the dews of the atmosphere; and I think it will be granted me, that perspiration could not have stalks, which most of these watery bubbles have. It is curious they should always turn milk-white.

Watery cryptogamiæ always turn milk white.

Flowers in hot climates (at least most of them) are thick in the petal, probably to guard the muscles from heat. To trace the effect of heat and cold in plants, to trace different climates in their productions, is the most delightful study; and one congenial to this, my present employment. Who can dissect the betula, and not be assured it is formed to endure the extremities of cold? Formed of 14 or 16 separate *rinds* (not barks) something like the skin I have before described, with a powder between each, how could frost approach it? Who can dissect the water lily with its large and prominent air vessels fluted, and with vacuities managed like the fish either to raise it in water or depress it, and not be sure it is formed to live in that element? Who can dissect the boletus, formed like a sponge, and guarded in its interior, without seeing it is to grow in caverns and under ground? or the *extreme* alpine plants, with their watery and open vessels, that they want little more than the dews of the atmosphere, and are not to depend on their rocky heights for food? But it is impossible long to dissect plants, and not see there are certain marks and indications, that give proof of all this.

Adaptation of plants to the places where they are to grow.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

Explanation of the Plates.

Plate V. figs. 3, 4.—Rose petal: fig. 3, upper skin, pink; fig. 4, pabulum, white.

Figs. 5, 6.—Violet petal: fig. 5, upper skin, purple; fig. 6, pabulum, white.

Figs. 7, 8, 9.—Crown-imperial and orange lily: fig. 7, first skin red; next, fig. 8, the net, green; and underneath, fig. 9, the pabulum of a dirty yellow white; which together make the flower of a beautiful rich orange.

Figs. 10, 11.—Jacobean lily: fig. 10, the upper skin, red; fig. 11, pabulum, yellow bubbles, imparting its fiery tint from the diviſion of light in them.

Fig. 12.—The heartsease, cut in thickness to show the rows at top and bottom of the petal.

Fig. 13.—The thick magnolia petal, cut in thickness, but not so much magnified.

Fig. 14.—Everlasting flower cut in thickness.

Plate VI, fig. 1.—The banner of the bean flower, *x x* spiral vessels.

Fig. 2.—A case holding the spiral vessels, with the nourishing vessels at the side.

Fig. 3.—Petal of the rose.

Fig. 4.—Wing of the lathyris flower.

Fig. 5.—Keel of a lathyris, *x* spiral vessels.

III.

Analysis of a Soil containing free Muriatic Acid, and Muriates of Soda, Lime, Alumine, Magnesia, and Manganese. By the Rev. WILLIAM GREGOR. *Communicated by the Author.*

Saline matter on a compact yellow loam. Situation.

AS I was passing through a lane, in my neighbourhood, some months ago, I was struck with the appearance of some saline matter on the soil, which formed the base of a hedge. This soil was what agriculturists, I presume, would denominate "a compact yellow loam."—It was not "*made ground*"; for the road had been evidently cut through it.—The spot is situate in the middle of a hill, of gradual ascent, in the parish of Merther, in the county of Cornwall, about a quarter of a mile from the river, which flows through Tressilian bridge, which is a tide river. But,

if

Nicholsons Philos Journal Vol XXVIII Pl VI p 180.

Corollas of Flowers.

Fig. 2

Fig. 3

Fig. 4

Fig. 5

if we may judge from circumstances of situation, this soil is removed beyond the reach of any direct or assignable influence, which the river can have upon it. I collected a sufficient quantity of this soil with a view of examining it at my leisure.

Treated with cold water,

It has a peculiar smell—distilled water, that had stood upon some of *it in the cold*, instantly reddened litmus paper. I extracted all that was soluble in this soil by distilled water. A portion of this solution was gradually evaporated in a retort: what came over into the receiver, examined from time to time, proved to be pure water, until the contents of the retort were reduced to a small compass, and began to assume a thick consistence: then the fluid, which distilled over, acquired the properties of an acid. And the modes of proof usually resorted to announced the acid to be the *muriatic*.

and part of the solution evaporated,

yielded muriatic acid.

Tests indicated also sulphuric acid, lime and other earths, and an oxide.

The common reagents, employed upon another portion of this solution, indicated the presence of the sulphuric and the muriatic acids, of lime and of some other earth or earths, and of the oxide of some metal. For liquid prussiate of potash, when dropped into it, caused an abundant greenish white precipitate; and spirituous tincture of galls, after a time, produced a turbidness, and a greenish gray matter was deposited.

Vegetable extract, sulphate of lime,

The solution is, at first, colourless and transparent: if it be evaporated, it gradually contracts a brownish hue: and vegetable extractive matter is deposited on the sides of the vessel. Sulphate of lime is first separated. When this substance has ceased to be produced, beautiful crystals of muriate of soda make their appearance: when these, by farther evaporation, have been exhausted, the remaining fluid cannot be brought to produce any other crystallized salt. It has a brownish tint, its transparency is in some measure impaired, and it strongly reddens paper stained blue by litmus.

muriate of soda.

The solution evaporated, and alcohol added.

If some of the solution be evaporated nearly to dryness, alcohol digested upon the mass separates sulphate of lime and muriate of soda. What the alcohol has dissolved consists of *muriates*, and vegetable extractive matter, for the fluid is brownish. It strongly reddens litmus paper.

If

If this solution is mixed with water, and ammonia dropped into it, an abundant brownish matter is thrown down; the clear decanted fluid, which is no longer rendered turbid by ammonia, yields up a small quantity of lime to the *carbonate* of ammonia. The matter, which ammonia has precipitated, becomes dark brown by ignition. Various experiments prove, that it consists of the *oxide of manganese*, and the earths *alumina* and *magnesia*. The salts, then, which this soil contains, appear to be *sulphate of lime*, and the respective *muriates* of *soda*, *lime*, *alumina*, *magnesia*, and *manganese*. It contains, also, *muriatic acid* in a disengaged state, and vegetable extractive matter.

Oxide of manganese, alumine, and magnesia.

Contents of the soil.

I wished to form some judgment as to the quantity and relative proportion of these salts in a given portion of the soil. With this view, I extracted all that was soluble by distilled water in sixteen ounces of soil previously freed as much as possble from stones. The solution was gradually evaporated, until no more sulphate of lime would appear. It was washed with alcohol mixed with water, collected and dried it = 13·75 grains. When, upon farther evaporation, the muriate of soda ceased to crystallize, the fluid was poured off, and the salt washed with alcohol. Upon evaporating the alcohol mixed with water, a small quantity of muriate of soda was obtained. The *whole quantity* of muriate of soda amounted to 117·25 grains. What was poured off from the last salt obtained was added to the residuary fluid: and ammonia was dropped into it, as long as it produced any effect. The browniſh precipitate was catched upon a filter. It required a much larger quantity of distilled water to edulcorate it sufficiently, than I could have expected. After exposure to a low red heat, it = 13·8 grains—carbonate of ammonia precipitated a small quantity of lime from the remaining fluid. This carbonate of lime was dissolved in muriatic acid; and in the state of dried muriate it scarcely amounted to two grains.

Analysis to ascertain their proportions.

The fluid, thus freed from earthy and metallic substances by the means of ammonia, was rendered slightly acid by pure nitric acid, and assayed by liquid nitrate of silver. The muriate of silver, which was precipitated, was sufficiently edulcorated. It was dried by being placed, during several

Muriatic acid.

several hours, in a silver crucible upon heated iron. It = 142·8 grains, equivalent to 22·8 of muriatic acid.

The 13·8 grains precipitated by ammonia were boiled to dryness in a ley containing 20 grains of potash; and the matter remaining in the silver crucible, which had resisted the action of the potash, after exposure to a low red heat, = 8 grains.

What the potash had dissolved was separated by muriate of ammonia from its solvent. It was *alumina*, and must have amounted to 5·8 grains. The 8 grains of residuum above mentioned became, by ignition, of a dark brown colour. Sulphuric acid did not apparently act upon it, until a small piece of sugar had been added. The sulphuric solution was decomposed by perfectly saturated carbonate of ammonia added to excess. After the precipitate thereby produced had been separated by the filter, and sufficiently edulcorated, phosphate of soda separated from the clear fluid the triple salt of magnesia first noticed by Dr. Wollaston. It = 3·9 grains, which, according to the calculation contained in Dr. Marcet's very ingenious and scientific analysis of the Brighton waters, indicate 0·74 of magnesia, which reduces the remaining substance to 7·26 grains. It had all the properties of *manganese*. What proportion of acid the muriates of alumina and manganese respectively contain, has not, I believe, been accurately ascertained. Alumine. Manganese.

I should not have considered the facts above detailed as being worthy of record, if I had not coupled them with the recollection, that, several years ago, I had examined a soil, which I have reason to think contained *precisely the same saline substances* as the subject of this letter. *Muriate of alumina* is not, indeed, mentioned in my notes upon it, but the results of the experiments which I *then* made agree so exactly with what I have recently observed, that I have not the least doubt, that muriate of alumina was present also, with the other salts. I did not then detect it, as in *the then state* of chemical means, it was not so easily discoverable. That soil contained also a free acid, and the muriate of manganese, with the other salts, which I have before enumerated. It differed, in one respect, from that which I lately examined: in having a much larger quantity of vegetable extractive matter mixed with it. The spot A similar soil observed several years ago

where

seventeen miles distant from this.

where that soil was found is about a mile from the town of Helstone, and it is distant above seventeen miles, in a direct line, from the spot described in this letter. It is possible, that this sort of soil may frequently occur, although it has, I believe, been unnoticed—the presence of a free acid and the muriate of manganese communicate to it properties, by which it may be quickly and easily *recognized.*

WILLIAM GREGOR.

Creed, Jan. 16, 1811.

IV.

Observations on the Oxides of Iron, with a Discussion of their Nature; by Mr. J. H. HASSENFRATZ*.

An accurate knowledge of the oxides of iron of great importance in chemistry,

IRON is the most useful of all metals; and is that which is most generally met with in all the substances we know. It is found in them either in the metallic state, or combined with oxigen. Chemists separate it in the state of oxide in most of their analyses: sometimes indeed they obtain it in the metallic form, but in this case it always retains a portion of the carbon employed in its reduction. The proportion of oxigen combined with the iron before and after analysis, in the oxide previously existing, or in the oxide obtained, has long been the subject of a discussion, which has occasioned errours in the estimations of products, from the little agreement that has existed and still exists among chemists.

and chemists differ much on the subject.

Berthollet asserts, that oxigen may combine with a metal in any proportion, from the minimum to the maximum;

The celebrated author of the Chemical Statics is persuaded, "that the proportions of oxigen in metals may vary progressively, from the limit at which combination first becomes possible, to that at which it has attained its highest degree†". A chemist whose opinions are of great weight, Proust, endeavours to establish the principle, that metallic oxides, and that of iron in particular, have but

* Annales de Chim. vol. LXIX, p. 113. For a valuable paper on this subject by Dr. T. Thomson, see our last volume, p. 375.

† Statique chem. tom. II, p. 370.

two

two degrees of oxidation; one at a minimum, the other at a maximum*. Thenard, known for the accuracy of his experiments, asserts, that there are three sorts of oxides of iron; the white, the green, and the red†.

Proust, that there is no union but in these two proportions: Thenard, that the proportions are definite, but may be more than two.

According to Berthollet, "the oxidation of metals, and the properties of the oxides they form, depend on the strength of their affinity for oxigen, their power of cohesion, their fusibility, their volatility, the degree of oxidation they are capable of attaining in consequence of these qualities, the condensation the oxigen undergoes in them, and the quantity of caloric it contains."

Berthollet's doctrines.

Now as the affinity of oxigen for metals, as well as their cohesion, fusibility, and volatility, must vary in proportion to the quantities of this substance already combined with them, it follows, that "*the combination of oxigen* (in metals) *may vary in them, and even indefinitely*, from the point when, the force of cohesion losing its preponderance, oxidation becomes possible, to that point where it ceases to be so, unless the mutual affinity of the two elements be assisted by some other affinity, which extends the limit of oxidation further."

Arguments of Proust.

Proust deduces his two degrees of oxidation from the colour of the precipitates obtained in solutions of iron on the addition of prussiates, alkalis, gallic acid, and hydrosulphurets.

Thoroughly saturated prussiate of potash precipitates from its solution iron at a minimum in the state of white precipitate, which afterward becomes green; and it precipitates iron at a maximum in the state of blue prussiate.

Alkalis precipitate oxidulated iron from its solutions in a green oxide, which gradually turns black: iron oxided at a maximum they precipitate red: and when a solution contains iron in both these states, the red is precipitated first, and the black afterward.

Gallic acid produces no change in solutions of oxide of iron at a minimum; that which is at a maximum it precipitates black.

* Ann. de Chim. vol. XXIII, p. 85.

† Ann. de Chim. vol. LVI, p. 57: or Journal, vol. XIV, p. 224.

Sulphuretted

Sulphuretted hidrogen produces no effect on solutions of iron at a minimum; while in solutions where the iron is at a maximum of oxidation it is decomposed, reducing the iron to the state of oxidule.

Those of Thenard.

Thenard distinguishes these three kinds of oxides by the action of alkalis, of gallic acid, and of prussiates, on solutions of iron; particularly on the solutions in sulphuric acid.

From their solutions in sulphuric acid alkalis precipitate the oxide of iron at a minimum white, the oxide at a medium green, and the oxide at a maximum red. These three oxides may be separated in succession from a solution in which they are mixed.

The gallic acid precipitates nothing from solutions of iron oxided at a minimum; from solutions in which the iron is oxided at a medium it throws down a green gallate; from that in which it is at a maximum a black gallate.

Lastly, the prussiates are blue and green with iron oxided at a minimum; green and blueish with that which is oxided at a medium; and blueish and blue with that which is at a maximum.

Bucholz considered Thenard's white oxide as a subsulphate,

Mr. Bucholz having made some observations on the white oxide of Mr. Thenard*; and attempted to maintain, that it was nothing but a combination of black oxide and sulphuric acid; the learned French chemist has pointed out a process, in a note published in the Bulletin des Sciences of the Philomathic Society†, by which the white oxide of iron may be obtained pure, and freed from sulphuric acid.

which the latter controverts.

The proportions of oxigen not given by Bertholiet,

The learned author of the Chemical Statics mentions none of the proportions of oxigen in oxide of iron. Indeed according to his principles he could have pointed out only those of the two extreme terms, that at which the combination becomes possible, and that at which it attains the highest degree; but to make known these two proportions we must be precisely acquainted with these two extremes.

or Thenard.

Thenard contents himself with giving the characters of

* Journal fuer die Chemie und Physick. Berlin. No. 12, p 721.

† Nouveau Bulletin, ann. 1808, No. 5, p. 96.

the

the three species of oxide he admits, without attempting to determine their proportion of oxigen.

Proportions assigned by Fourcroy.

The celebrated author of the System of Chemical Knowledge, Fourcroy, announces, that the proportions of oxigen, deduced from the experiments of the illustrious Lavoisier, are, for the oxidule, from 25 to 27 parts of oxigen to 100 of iron; and for the oxide, from 40 to 49*.

Proust.

Proust, who has defended with so much acumen two degrees of saturation of iron only, the one at a minimum, the other at a maximum†, carries the quantity of oxigen in the former as far as 28 to 100 of iron, in the latter to 48.

and Bucholz.

Mr. Bucholz, a German chemist of deserved reputation, who has lately attempted to determine the proportion of oxigen in the two degrees of oxidation‡, states the minimum proportion of oxigen at 30 to 100 of metal, and the maximum at 42.

The diversity of opinions between them led to the following examination of what has been done on the subject.

From this diversity of opinion on the degrees of oxidation, and the proportions of oxigen in the different oxides, between chemists equally celebrated for their skill and their precision in analysis, I conceived it might be of use to give a succinct account of the attempts that have been made to solve this question, which is of essential importance to chemists. This is the only motive, that has led me to publish the following abstract of the experiments that have been undertaken, to attain a knowledge of the oxide of iron, and determine its different degrees of oxidation.

Principal methods employed for oxiding iron.

Of the methods of oxiding or disoxiding iron, by means of which the proportions of oxigen, that combine with this metal, may be determined, the six following are the principal that have been employed: 1st, oxidation by the action of air and heat: 2d, the reduction of oxides of iron, either by the action of heat alone, by that of heat and

* Syst. de Conn. chim. tom. VI, p. 160 and foll. English transl. p. 214 and fol.

† Ann. de Chim. tom. XXIII, p. 85. Journal de Phys. 1804, tom. II, p. 33.

‡ Jour. fuer die Chem. and Phys. Berlin, No. 12, p. 696. and fol.: Journ des Mines, No. 131, p. 361 and fol.: or our Journal, vol. XXV, p. 353.

carbon,

carbon, or by that of heat and hidrogen: 3d, oxidation by other metallic oxides: 4th, by water; 5th, by acids; and, 6th, by nitre. I shall examine the results obtained by each of these methods separately.

1st Method. *Oxidation by air and fire.*

Simple calcination by Schæffer,

Schæffer appears to be the first chemist, who observed, that iron increased in weight by exposure to the air and heat*. He remarked, that this increase, after a complete calcination, was one third the weight of the iron, consequently 33·3 to 100 of metal.

Guyton,

Guyton repeated Schæffer's experiment†. He found, that iron filings calcined in a Hessian crucible, under the muffle of an open furnace, increased $\frac{1}{3}\frac{1}{5}$, or 27·4 to 100 of iron. Steel filings, burned in the same manner, increased 38·58 per cent.

Lavoisier,

Lavoisier, calcining iron filings in the same way, obtained four oxides, which had increased respectively 30, 32, 32·41, and 33 on the 100 of iron‡.

Darso,

Mr. Darso calcined iron repeatedly, taking it out, and triturating it in a mortar, after each time of exposing it to the fire. By these means he obtained oxides, which increased successively 20, 30, 36, 40, 45, 50, and even 56 on 100 of iron filings§.

Hassenfratz,

I repeated with much care the experiment of Mr. Darso, but could never obtain more than 42 on 100 of iron filings without stirring them while calcining, and 45 with stirring them‖: which leads me to suspect some errour in Mr. Darso's results.

Bucholz,

Bucholz calcined iron filings in a crucible, exposing them to a red heat, and stirring constantly. The iron acquired an increase of 24 to 100 of the metal.

and Lavoisier again.

Lavoisier, repeating the experiment of Ingenhousz, who burned fine iron wire in oxigen gas, burned iron turnings

* Swedish Transactions, 1757.

† Digression académique, Essai sur le Phlogistique, p. 8.

‡ Mém. de l'Ac. des Sciences, 1782, p. 525 and fol.

§ Journ. de Phys. 1806, tom. II, p. 294: or Journal, vol. XVII, p. 221, 267, 328.

‖ Ann. de Chim. vol. LXVII, p. 309: or Journal, vol. XXVI, p. 147.

in

in a vessel under a jar filled with oxigen gas, and standing over mercury. He found 100 parts of iron increased to 135 or 136 in one experiment, and to 132·414 in another*.

This difference of results in similar experiments shows the great difficulty of executing them properly: and among all that have been mentioned mine is the only one, in which the iron appears to have been carried to its maximum of calcination; as that of Mr. Darso, though indicating a larger proportion of oxigen, I conceive to have been inaccurate. In my experiments iron oxided at a maximum appears capable of being carried as far as 45 of oxigen to 100 of iron, or about 31·1 of oxigen to 68·9 of iron, which may be called in round numbers 31 per cent of oxigen. Remarks on these experiments.

As to the other experiments, which carry the oxidation to 24, 27, 30, 33, and even 38, we may presume, that portions of metal not yet completely calcined still remained among the iron, for on triturating the iron, after each calcination it was observed, that *little nodules of iron remained a long time, which the oxigen had not attacked.*

2d Method. *Disoxigenation of the oxide by heat alone, and by heat and carbon or hidrogen.*

Attempts have frequently been made, to carry back the oxide of iron from its maximum by calcining it strongly, either alone, or with substances that have a stronger affinity than it has for oxigen. Attempts to abstract oxigen from the oxides, by heat alone,

Mr. Berthollet asserts, that, on calcining red oxide of iron in porcelain tubes, it gave out no portion of oxigen when exposed to a powerful heat, though its colour appeared to change from red to black†. by Berthollet,

Fourcroy too asserts, that on calcining oxide of iron no oxigen was evolved; but he pretty constantly obtained carbonic acid, when the oxide assumed a black colour‡. Fourcroy,

Mr. Bucholz, in the paper already quoted, observes, that red oxide of iron being exposed in Hessian crucibles to the action of a powerful fire, the weight of the crucibles and oxide was diminished a few grains. This loss, if Bucholz,

* Traité élém: de Chim. tom. I, p. 41.

† Journ. de Phys 1805, vol. II, p. 362.

‡ Mém. et Obs. de Chim. p. 102, 103.

it

it can be ascribed to oxigen disengaged from the iron, would be about 6 per cent.

and Fourcroy again. Fourcroy, calcining 144 grains of oxide of iron in a porcelain crucible hermetically closed, found that the apparatus had lost 50·6 grains, though the lute was fused, and the lid united to the crucible by an earthen glass.

Remarks on these experiments. These different results in experiments, that may be considered as similar, leave some doubts on the conclusions, that Bucholz draws from his. In fact, the crucibles he employed were vitrified; which shows, that they had been exposed to a higher temperature than that in which they were made. This increased temperature of the crucibles therefore leaves the cause of the loss of weight, which the crucible and oxide had undergone, a matter of doubt. It may as well be ascribed to water remaining in the earth of the crucible, as to oxigen disengaged from the iron.

Attempts with substances containing carbon, by Bucholz; Bucholz also endeavoured to diminish the oxigen in the oxide of iron by triturating and heating it with substances containing carbon; for instance, carbonate of ammonia, supertartrite of potash, wax, lampblack, and succinate of ammonia. With the first three the oxide experienced only an imperceptible diminution and reduction; and with the others the diminutions were very variable. With the lampblack one portion of the oxide had undergone no change, while another was reduced to the metallic state.

The oxide and oxidule of iron may be completely disoxidated and reduced by heating or fusing them with substances containing carbon and hidrogen.

with charcoal, by Hassenfratz; At Moustier we fused with charcoal a variety of oxidulated iron in dodecaedral crystals from a mine in the valley of Aost. This ore, which contained 0·98 of oxidule and 0·02 of silex, yielded a button weighing 0·76. If no silex had been present it would have weighed 0·775. Supposing the metal to have retained 0·005 of carbon, it would follow, that this oxidule contained 30 of oxigen to 130 of iron. Two oxidules from the isle of Elba, one assayed at the laboratory of the Practical School of Mining, the other at the laboratory of the Council of Mines, registered No. 263, yielded 0·72 of iron. The first contained 0·05 of foreign

foreign matter: and if we may suppose the second to have contained the same quantity, and each of the buttons to have retained 0·01 of carbon, it would follow, that these oxidules were composed of 33 oxigen to 100 of iron.

and with hidrogen gas, by Priestley, Chaussier, A. Berthollet,

Priestley and Chaussier both disoxidated iron by raising it to a high temperature, and thus exposing it to the action of hidrogen gas. Amédée Berthollet has since announced the same result in a paper read to the Institute for 1807. As neither of these gentlemen has pointed out the diminution of weight of the oxidule in these experiments, Mr. Charbaut, a pupil at the Practical School at Moustier, and myself repeated them in the summer of 1807. We found, that 11·58 grains of oxidule of iron from the island of Elba lost in reduction 3·34 grains; whence it would follow, that 140·53 of oxidule contained 40·53 of oxigen to 100 of iron; supposing it to be perfectly pure, which is very probable; though of this we are not certain, as we did not analyse it.

Charbaut, and Haffenfratz.

Remarks on these experiments.

From these experiments we may conclude, that it is very difficult, if not impossible, to carry back the oxide, either by means of heat alone, or of heat and carbon, to the state of oxidule. From the reduction of the oxidule of the valley of Aost by charcoal it would appear to consist, taking the nearest round numbers, of 77 iron and 23 oxigen.

At the conclusion of these observations we will examine, to what the difference between the oxidules of Aost and Elba is ascribable.

3d Method. *Oxidation of iron by metallic oxides.*

Oxidation of iron by oxides of other metals,

Iron has not yet been oxided by the help of any other metallic oxides, that have less affinity than itself for oxigen, but those of arsenic and mercury; because these two metals, when disoxidated, evaporate at a high temperature.

by Guyton,

Guyton fused in a closed crucible equal parts of iron and oxide of arsenic. The latter metal evaporated, and the iron was oxided. The increase of weight was 27·1 on 100 of iron.

and Lavoisier.

Lavoisier fused iron both with oxide of arsenic and oxide

* Dig. Acad. Dict. sur le Phlogistique, p. 189.

of

of mercury. He found, that in these two fusions the iron increased from 35 to 40 per cent. In the table which follows his paper he gives the increase of iron by arsenic as 30 of oxigen to 100 of metal; and in his Elementary Treatise of Chemistry he quotes an experiment, in which iron treated with oxide of mercury gained 32 per cent.

No accurate inferences to be drawn from these.

It is not easy to deduce from these experiments any positive conclusion respecting the proportion of oxigen in the two degrees of oxidation, because, 1st, the quantities of oxigen combined vary too much: 2dly, the state of the oxide after oxidation was not accurately ascertained: 3dly, it is not certain, that the arsenic, which has a great affinity for iron, did not carry away some of it when volatilized. It appears too, that Guyton, in his experiment, did not mix a sufficient quantity of oxide of arsenic with the iron; for, according to the experiments of Proust*, the white oxide of this metal contain but 33 of oxigen to 100 of arsenic; whence the 100 parts of oxidecould only have yielded about 25 of oxigen to the iron.

4th Method. *Oxidation of iron by water.*

Oxidation of iron by water. Martial ethiops.

The black oxide has long been prepared in the shops, under the name of martial ethiops, by putting iron filings into a bottle full of water, shaking the mixture from time to time, and opening the bottle after every shaking, to let out the hidrogen gas evolved. To accelerate the process, Rouelle proposed to acidulate the water with vinegar, Croharé with a little nitric acid†.

Lavoisier.

Lavoisier found, that iron filings gained an increase of 30 or 35 per cent by agitation in pure water; and the same if the water were acidulated‡.

Cavazzely.

Cavazzely, on oxiding iron by means of water, obtained an oxide increased 35 per cent on the iron employed§.

Bucholz.

Bucholz, repeating the experiment of oxidation by water alone at the common temperature of the atmosphere,

* Journ. de Phys. 1799, tom. II, p. 150.

† Ann. de Chim. vol. XXXI, p. 333 and fol.

‡ Ac. des Sciences, 1782, p. 543 and fol.

§ Ann. de Chim. vol. XLIII, p. 94.

remarked,

remarked, that the oxide was covered with a crust of yellow oxide, while another part was in the state of black oxide.

The great variation in the weight of the oxide obtained by this method, and the observation of yellow oxide formed on its surface, show, that nothing can be deduced from experiments of the kind. These experiments of no account.

Lefebvre, Stoutz, and myself, when in Carinthia, satisfied ourselves, that red hot iron immersed in water decomposed it; the iron being oxided, and hidrogen evolved*. Water assisted by heat.

The illustrious Lavoisier, taking the hint from this experiment, heated little bits of iron twisted spirally in a copper tube, and passed the vapour of water through them while red hot. The water was decomposed, and the iron oxided. The iron had increased in weight 31·38 per cent. The hidrogen gas, which he collected and weighed, was found to be in the proper proportion for forming water with the oxigen combined†. Lavoisier.

Bucholz, repeating the experiment of Lavoisier by passing the vapour of water through iron filings heated red hot in a close vessel, obtained a black oxide containing 76·7 of iron and 23·3 of oxigen. Bucholz.

The result obtained by Lavoisier appears accurate, and that of Bucholz rather too low. It is to be regretted that this process was not oftener repeated, but I shall notice this result again, when recapitulating those that may be considered as accurate.

(To be concluded in our next.)

V.

Some Remarks on the Observations and Experiments of Mr. Murray on the Nature of Oximuriatic Acid, and its Relations to Muriatic Acid. *By Mr.* JOHN DAVY.

To Mr. NICHOLSON.

SIR,

IT is generally admitted as a fundamental principle of modern chemistry, that all bodies not yet decompounded Substances not decomposed to

* Mém. de l'Acad. des Sciences, 1781, p. 475.

† Traité élém. de Chim. tom. I, p. 92 and fol.

be deemed simple.

are to be considered as simple substances. In this respect the phlogistic principally differs from the antiphlogistic doctrine: the former is hypothetical, the latter theoretical.

Difference between the phlogistic and antiphlogistic doctrine.

The phlogistic hypothesis in its most perfect form, as advanced by Mr. Cavendish, could not be confuted: it was simple, luminous, and perfectly well adapted to explain all the phenomena of chemistry; but it was still an hypothesis. Theory is a history of generalisation and facts, hypothesis is a series of suppositions. Mr. Murray does not observe this distinction, he makes promiscuous use of the two words. Sometimes he speaks of the theory, and at other times of the hypothesis of Mr. Davy. I think it necessary to point out what to me appeared a loose mode of expression. It is of main importance, that expressions of facts be not misrepresented. It is of great consequence, that things be not termed notions—that theory be not considered as speculation.

Difference between theory and hypothesis.

Mr Davy's conclusions not hypothetical.

Before I examine the objections of Mr. Murray to Mr. Davy's conclusions, I shall endeavour to show, that these conclusions are not tainted by the slightest admixture of hypothesis. With this object in view I beg leave to recapitulate a few leading circumstances.

Formation of muriatic acid gas from oximuriatic and hidrogen.

Muriatic acid gas is formed by detonating together equal volumes of oximuriatic gas and hidrogen gas; and, if the experiment is correctly made, there is no condensation attending their union*.

Metals form peculiar compounds with oximuriatic gas:

The metals combine immediately with oximuriatic gas, and form with it a peculiar class of compounds, many of which are decomposed by water; and when this occurs, a metallic oxide and muriatic acid gas are always produced.

and the same compounds with muriatic acid gas, hidrogen being set free.

When the metals of the fixed alkalis, or the common metals, mercury, iron, tin, and zinc, are heated in muriatic acid gas, hidrogen gas equal to one half of the volume of the acid gas consumed appears free, and substances are produced exactly similar to those formed by the direct union of the same metals with oximuriatic acid. In these experiments

The quantity of hidrogen not at all af-

there is no difference in the proportion of hidrogen gas li-

* I have seen this experiment made a number of times, in vessels that had been exhausted, and over mercury, and with similar results.

berated

berated: it is not less, when the acid gas has been previously exposed to the action of salts having a strong attraction for water: nor is it greater in relation to the quantity of muriate formed, when liquid muriatic acid is used. fected by the presence of water.

When metallic oxides are acted on by oximuriatic gas, oxigen gas is evolved; and when acted on by muriatic acid gas, water is produced. In the one instance the oxigen disengaged is the exact quantity contained in the oxide; and in the other case, the water produced has as much hidrogen, as existed in the muriatic acid gas, and as much oxigen as the metallic oxide contained. Oximuriatic gas seizes the metal of an oxide, and expels its oxigen; which, if muriatic acid gas be employed, combines with its hidrogen, and forms water.

Such I conceive are the principal facts, which constitute the foundation of my brother's theory, that oximuriatic gas is a simple body. He combines this gas with hidrogen gas and forms muriatic acid gas. In his theory muriatic acid gas is a compound of oximuriatic gas and hidrogen. He combines oximuriatic gas with sulphur, phosphorus, and the metals: and in his theory, the resulting substances are compounds of the inflammable and metallic bodies respectively, and oximuriatic gas. Here we perceive no supposition, but a simple expression of facts, and this I humbly conceive is pure and genuine theory. Mr. Davy's theory.

Having stated my opinion respecting Mr. Davy's views, I shall proceed to examine Mr. Murray's. This gentleman is of the old opinion, that oximuriatic gas is a compound body, consisting of muriatic acid and oxigen; that the muriates, substances formed by the combustion of the metals in oximuriatic gas, are composed of muriatic acid and metallic oxides: and likewise, that the constituents of the compounds resulting from the combustion of sulphur and phosphorus in oximuriatic gas are the respective acids of these two bodies and muriatic acid. Mr Murray's, or the old theory.

Let it be kept in remembrance, that Mr. Murray does not mean, when he speaks of muriatic acid, the ponderable part of muriatic gas, which combined with ammonia, constitutes muriate of ammonia. The idea he attaches to the word is very different, for he says, that water is absolutely necessary to the existence of muriatic acid in the gaseous state; that muriatic acid has not yet been decomposed; and that it has an important and an anomulous relation to wa- Mr. Murray's muriatic acid not the whole ponderable part of muriatic acid gas,

ter, the investigation of which presents an interesting subject for inquiry.

but something contained in it,

What, it may be naturally asked, does Mr. Murray understand by muriatic acid? as he has given no definition of the word, I cannot say positively in what sense he uses it. This is certain, it does not signify muriatic gas, but, I should conceive, *that* part of muriatic gas, which combines with metallic oxides, and with the phosphoric and sulphuric acids. But the question now is, what is this

which has never been obtained separate,

peculiar substance? Has Mr. Murray examined it in its insulated state? has he described its properties? or has any chemist ever obtained it? The answer I may venture to assert is in the negative. To speak plainly my sentiments, muriatic acid gas, on Mr. Murray's hypothesis, appears to me to be a compound of water and an unknown basis, and the muriates compounds of this unknown basis and

and is therefore hypothetical,

metallic oxides. And *this*, if I am not greatly mistaken in my opinion, is speculation in the strictest sense of the word.

It is evident, that, whilst the basis is unknown, it is merely an imaginary substance. If this hypothesis should be preferred to Mr. Davy's theory, there should be for the sake of consistency a revolution of chemical doctrine.

and on a par with phlogiston,

Phlogiston should triumph, and recover all it's former dignities. The arguments advanced in support of the simple nature of muriatic acid, might be used in *defence* of the compound nature of the metals. It has been already observed, that Mr. Cavendish's hypothesis of this kind is

Mr Cavendish's hypothesis.

fully adequate to explain all chemical phenomena. This illustrious philosopher considered the metals and inflammable substances as probably compounded of unknown bases and phlogiston (hidrogen), and the metallic oxides and acids as probably compounds of unknown bases and water*.

Examination of Mr. Murray's objections to Mr. Davy's theory.

I shall no longer delay examining the objections made by Mr. Murray to Mr. Davy's theory, that oximuriatic gas is a simple body, having relations similar to those of oxigen to the metals and inflammable substances. Mr. Murray

Improbability

remarks, "that there is some improbability in the hypo-

* Phil. Trans. for 1784.

thesis

thesis of two acidifying principles distinct from each other, but exerting similar chemical agencies," p. 139. It is generally admitted, that the only legitimate mode of reasoning in the experimental sciences and in chemistry in particular, is that of induction. We are not to judge of Mr. Davy's views, I humbly conceive, by considering their probability or improbability a priori, but by a reference to facts. I beg leave to take this liberty. Oximuriatic acid is attracted like oxigen by the positive and repelled by the negative pole of the Voltaic battery. Combined with hidrogen oximuratic acid constitutes muriatic acid. Combined with sulphur, phosphorus, and some of the metals, it forms peculiar compounds, possessing the property of neutralizing ammonia*. It may be objected, that these compounds do not change vegetable colours from blue to red. Granted. Neither does prussic acid. But this change is affected by sulphuretted hidrogen, which there is every reason to believe contains no oxigen, independent of that small portion which probably exists in sulphur. The action of oximuriatic acid on vegetable colours, and indeed all the chemical relations of this substance, are anomalous on the old hypothesis; for the sulphuric acid in its most concentrated state very slightly reddens dry litmus paper: and I find, that acetic acid does not redden it at all, when freed from water by long digestion with fused muriate of lime. Are not all these facts sufficiently decisive in their bearings to authorise the conclusion, that oximuriatic acid is an acidifying principle, similar to oxigen?

of two acidifying principles of no weight

Analogies between oxigen and oximuriatic acid.

That the compounds of the latter do not redden vegetable blues no objection.

To avoid all verbal controversy, I candidly acknowledge, that I consider the acidifying principle as a mere arbitrary term. Oxigen strictly speaking can with no more propriety be said to be the cause of the acidity of sulphuric acid, than sulphur. But if the term is to be still continued in use, we can only apply it to oxigen and oximuriatic acid, as these two substances are the only ones known to enter into the composition of a great variety of bodies having analogous acid properties.

Properly no acidifying principle.

* The only alkali on which they can act without double decomposition.

Mr.

Want of action between charcoal and oximuriatic gas.

Mr. Murray is of opinion, that the want of action between charcoal and oximuriatic gas is an anomaly in Mr. Davy's theory, which does not exist in the old hypothesis. To account for this fact he has recourse to a predisposing affinity, and to the supposition, that water is necessary to the existence of the muriatic and carbonic acids in a gaseous state. And he remarks, that, as water was not present when the experiment was made on the ignition of charcoal by Voltaic electricity in oximuriatic gas, it followed, that no alteration could be produced, that no oximuriatic gas could be decomposed, or any muriatic or carbonic gas formed. This reasoning would have been less unobjectionable, were it less hypothetical. Mr. Murray does not prove, that the muriatic and carbonic gasses contain water; he supposes that they do. The only fact advanced in favour of this constitution regards carbonic acid gas. It is well known, that the native carbonate of barytes differs from the artificial in being indecomposable by heat alone; and that this difference ceases, when the action of fire is assisted by steam or aqueous vapour. Mr. Murray supposes, that steam produces the decomposition by entering into union with the carbonic acid, and forming carbonic acid gas. I should conceive, that he would not have adopted this supposition, were he acquainted with Mr. Berthollet's observations on barytes. From this gentleman's experiments it appears, that the water does not combine with the acid, but with the earth, which it converts into a fusible hidrate, and thus liberates the carbonic acid, which immediately escapes in the gaseous state*.

Water supposed to be necessary to carbonic acid gas from its assisting the decomposition of native carbonate of barytes;

but this it does by converting the earth into a fusible hidrate.

Though Mr. Murray has not endeavoured to demonstrate the presence of water in muriatic acid gas, he has attempted to show, that this probably is the case. When the acid gas has been exposed to the action of substances strongly attracting water, Mr. Murray observes, "it can only be affirmed, that a portion of water is abstracted from it, not that the whole of it has been withdrawn. As the water is removed from the acid gas, the affinity by which the remaining quantity is retained must prodigiously in-

Argument of Mr. Murray for the existence of water in muriatic acid gas

* Memoires d' Arcueil, tom. ii, p, 17.

crease

crease in strength, until it equals or exceeds that of the substance attracting it: when of course its abstraction must cease," p. 135. If this statement be correct, it necessarily follows, that muriatic acid may exist as a gas in combination with different proportions of water. On the supposition that it does contain water, experience proves the contrary—for the proportion of hidrogen gas produced from muriatic acid gas acted on by different metals is always the same, whether it has been exposed to the influence of drying salts or not. I readily admit, that muriatic acid gas has important relations to water, and a strong attraction for this fluid; so too has fluoboracic acid gas: but I cannot allow the same relations and attraction to the muriatic acid of Mr. Murray—an imaginary body, that I confess I am altogether ignorant of. Moreover the strong affinity of muriatic acid gas for water is no more in favour of water being one of the constituents of this gas, than the strong attraction of potassium and sodium for oxigen is for the existence of oxigen in these metals.

proves that it may contain different proportions:

but metals in all cases evolve from it the same quantity of hidrogen.

Certain gasses have important relations to water.

When Mr. Davy concludes, that oximuriatic gas has relations similar to oxigen, I do not understand, that the expression implies an exact similarity between the attractions of these two substances, but merely a general analogy, such as the metals, or the inflammables have to each other, which allows of their being classed together.

Oxigen and oximuriatic gas analogous, but not in all respects similar.

If I may be permitted to give my opinion, I should say, we have no right to expect from a theory the explanation of ultimate facts. And the fact in question (the want of action between charcoal and oximuriatic gas,) is one of this kind. It is one of those, which constitute as it were the axioms of the science. For my own part I am glad, that it is not tortured by hypothetical explanation.

Explanation of ultimate facts not to be expected from a theory.

Having considered the reasons adduced by Mr. Murray for dissenting from Mr. Davy's theory, I shall proceed to examine the results of the particular experiments, which this gentleman considers more favourable to the old hypothesis than to the new views.

Mr. Murray's experiments examined.

Mr. Murray is of opinion, that the experiments of Mr. Cruickshank on the analysis of carburetted hidrogen gasses by oximuriatic gas are hostile to Mr. Davy's theory: as

Conversion of oximuriatic and carburetted hidrogen

when

gasses into carbonic and muriatic acid,

when the proper proportions of the two gasses were used, there was found to be a complete conversion of the whole into carbonic and muriatic acid gas. In answer to this

not always the fact.

objection it is only necessary to observe, that I have carefully repeated some of these experiments, making the detonations over recently boiled mercury, and never obtained carbonic acid gas though oximuriatic gas in great excess was employed. No regard was paid to the influence of water by the original author, therefore nothing can be inferred from the experiments of Cruickshank, but what is perfectly agreeable to Mr. Davy's theory. The presence of water is neglected by Mr. Murray: to him the only cause of ambiguity in the result appears to arise from the small portion of oxigen, which he supposes these gasses contain, and from which carbonic acid gas must be formed, when the hidrogen is separated by oximuriatic gas.

Action of oximuriatic gas on carbonic oxide

To avoid this source of ambiguity he has had recourse to carbonic oxide. He has repeated the experiment of Mr. Cruickshank on the action of oximuriatic gas on this compound, and has confirmed his results. He has very satisfactorily proved, that no change takes place in the two gasses when water is entirely excluded; and that, without the agency of water, no carbonic or muriatic acid gas is formed. The conclusion he draws from this fact is in favour of the old hypothesis. He takes it for granted, that "water is not decomposed by oximuriatic gas:" oxigen therefore he observes can only be afforded to the carbonic oxide to convert it into carbonic acid by the decomposition of the oximuriatic gas. It appears to me, that, even on Mr. Murray's own statement, his conclusion is gratuitous.

an insufficient proof,

It is not sufficient for his purpose to prove, that some carbonic acid is formed, but that the whole of the carbonic oxide

since it may be otherwise accounted for.

is converted into this acid. For there is no improbability in the supposition, that some carbonic acid might be produced by the formation of a triple compound of oximuriatic acid, hidrogen, and carbon. The oil resulting from the union of oximuriatic gas and olefiant gas is a substance of this kind; and we know of several analogous cases in chemistry. Independent of what has been just said, have we not the most indubitable evidences of the decomposition of

water

water by oximuriatic gas? Pass this gas and steam together through a tube heated to redness, oxigen gas will be produced, and muriatic acid gas formed. Detonate this gas and hidrogen gas together, and a similar formation of muriatic gas will take place. With these facts in view, who can hesitate in asserting, that water is decomposed by oximuriatic gas, that its hidrogen is attracted by this substance, and its oxigen consequently set free? Water decomposed by oximuriatic gas.

Since charcoal has no action on oximuriatic gas, is there any anomaly in the want of action between this gas and carbonic oxide? and since oximuriatic gas alone decomposes water, is there any thing unexpected in this effect being more readily produced by the joint attractions of oximuriatic gas and carbonic oxide?

Mr. Murray, to obviate every objection that might be raised to the formation of carbonic acid gas when water was present, made mixtures of carbonic oxide, hidrogen, and oximuriatic gas, over mercury, which were exposed to the solar light, or detonated by an electric spark. He used water for absorbing the muriatic gas formed, and constantly found great part of the carbonic oxide converted into carbonic acid. Carbonic acid produced from carbonic oxide, hidrogen, and oximuriatic gas over mercury.

I have assisted Mr. Davy in repeating these experiments; our mode of operating differed principally in this respect, we used ammoniacal gas instead of water for removing the muriatic gas, and we have obtained, probably in consequence of this, very different results. I beg leave to transcribe the account of one of the experiments, given by Mr. Davy in a note at the end of his Bakerian lecture for 1810. Different results obtained by Mr. Davy.

"We found that 7 parts of hidrogen, 8 parts of gaseous oxide of carbon, and 20 parts of oximuriatic gas, exploded by the electric spark, diminished to about 30 measures, and calomel was formed on the sides of the tube. On adding dry ammonia in excess, and exposing the remainder to water, a gas remained, which equalled more than 9 measures, and which was gaseous oxide of carbon, with no more impurity than might be expected in the air in the gasses and the nitrogen expelled from the ammonia." In another experiment conducted in the same way as the last, a mixture of 22 measures of oximuriatic gas, of 8 hidrogen An experiment of his. Another experiment.

gen gas, and of 10 carbonic oxide, was inflamed by an electric spark over recently boiled mercury. There was a diminution produced equal to 6 measures, and some calomel formed. After the addition of ammoniacal gas, and the subsequent addition of water, there remained rather more than 13 measures of unabsorbable air, which burnt with the same coloured flame as carbonic oxide. We have made this experiment several times, and always obtained similar results*.

Difference of the results accounted for.

It appears therefore evident, that the formation of carbonic acid in Mr. Murray's experiments must either have been owing to his having used water, or to the gasses employed containing oxigen. Indeed he states, that there was a residue of common air in one of the experiments, which he considers of the most decisive nature. Now it is well known, that carbonic acid gas is formed, when carbonic oxide and atmospheric air are detonated together. We are not at a loss therefore to account for the difference of the results of Mr. Murray's experiments, and of those which we obtained.

In favour of Mr. Davy's theory.

The former were in opposition to Mr. Davy's theory, and on repetition they are not found to oppose it; and thus, instead of invalidating Mr. Davy's conclusions, they have quite a contrary tendency; they prove, that his theory will bear close examination, and that the facts, which at first

* The carbonic oxide used was procured from native carbonate of barytes and iron filings, and the oximuriatic gas from hyperoximuriate of potash by strong muriatic acid; the first portions only were preserved, which appear to be nearly pure oximuriatic gas.

New discovered compound of oximuriatic gas and oxigen.

Since this paper was written, Mr. Davy has discovered that there exists a very curious gaseous compound of oximuriatic gas and oxigen, which may be formed from hyperoximuriate of potash by diluted muriatic acid, assisted by a low heat: Possibly Mr. Murray's gas may have contained this substance, which we have found will convert carbonic oxide into carbonic acid. When *pure* oximuriatic gas is used, the results are as above stated.

From the particulars relating to this compound of oximuriatic gas and oxigen, described by Mr. Davy in his paper on the subject just read to the Royal Society, there are such additional proofs of oximuriatic gas being an undecompounded body, as must, I conceive, carry conviction to those who still adhere to the old hypothesis.

appear

appear inimical to it, do, when minutely investigated, afford additional proof of its accuracy, and of the incorrectness of the old hypothesis.

Mr. Murray's experiments with oximuriatic and sulphuretted hidrogen gas.

Mr. Murray was confirmed in the belief of the existence of oxigen in oximuriatic gas from the results of some experiment which he made with this substance and sulphuretted hidrogen gas. He states, that when a small quantity of the latter is admitted into a globe containing a large quantity of the former, no deposition of sulphur takes place; and only a slight cloudiness is produced, which soon disappears, and leaves the gas as transparent as at first. It is Mr. Murray's opinion, that sulphuric acid, or probably sulphurous acid gas, is formed, besides the muriatic acid gas. I shall silently pass by the experiments of this gentleman in which water interfered, for obvious reasons. And before I comment on the above results, I ask permission to mention those, which were obtained by Mr. Davy and myself in repeating this experiment in different forms.

These repeated in different forms.

On adding 3 measures of dry sulphuretted hidrogen to 6 measures of oximuriatic gas in a jar over recently boiled mercury, there was a vivid inflammation, a condensation equal to 2 measures, and a slight deposition of sulphur. The colour of the gas was not entirely destroyed, but it soon disappeared, and the sulphur at first precipitated was no longer visible; but its place was supplied by a fine amber coloured dew, similar to the oximuriate of sulphur of Dr. Thomson, and there was a farther condensation equal to 2 measures. The muriatic gas thrown into the air had not the slightest smell of sulphurous acid gas, but it had the peculiar one of oximuriate of sulphur; and the water, by which some of it had been absorbed, produced only a slight cloudiness with muriate of barytes.

Results.

From these results it appears, that muriatic acid gas and oximuriate of sulphur are the only substances formed.

Oximuriate of sulphur rises in vapour in muriatic acid gas.

We have ascertained by direct trial that oximuriate of sulphur rises in vapour in muriatic acid gas. For the acid gas, after confinement over mercury in contact with this substance, had its very peculiar smell, and affected muriate of barytes with a cloudiness.

Gaseous triple

From Mr. Murray's account it appeared probable, that a gaseous

compound probably formed.

a gaseous triple compound of oximuriatic acid, hidrogen, and sulphur, was formed in his experiments, as he takes no notice of the appearance of any condensed fluid on the sides of his vessels. To ascertain whether this was the case, we have operated upon large quantities. Three cubic inches of sulphuretted hidrogen, allowed to pass from a receiver with suitable stop-cocks into a tube containing 6 cubic inches of oximuriatic gas, inflamed as it entered; a very slight cloudiness was produced, which soon disappeared; and there was an immediate formation of oximuriate of sulphur, which condensed on the interior of the tube, and was not redissolved by the muriatic acid gas, though heat was applied.

Experiment to ascertain this.

Precipitations of muriate of barytes accounted for.

In respect to Mr. Murray's results I have only to observe, that as he absorbed the muriatic acid gas by water in the very vessel in which the experiment was made, the solution, thus formed, should, as it indeed did, occasion a copious precipitate with muriate of barytes. It is now a well known fact, that the sulphuric and muriatic acids are formed by the decomposition of oximuriate of sulphur by water.

Habits of oximuriatic with sulphurous acid gas.

It may not perhaps be impertinent to the subject to state, that the relation of sulphurous acid gas to oximuriatic gas is precisely analogous to that of the gaseous oxide of carbon. I ascertained some months ago, that the two gasses previously dried admitted into an exhausted glass globe remain together without any alteration taking place; but that if a little water be present, muriatic acid gas and sulphuric acid are quickly formed.

The old hypothesis not sufficient, to explain all the phenomena.

I have examined with that freedom, which the subject required, Mr. Murray's objections to Mr. Davy's theory, and his ingenious defence of the old hypothesis. An hypothesis, I humbly imagine, inadequate, if admitted, to explain all the phenomena, and inconsistent with many well authenticated facts. To illustrate this assertion, let it be granted, that Mr. Murray, avoiding all sources of inaccuracy, obtained carbonic acid gas by detonating his mixture of oximuriatic gas, hidrogen gas, and carbonic oxide. Mr. Murray allows, that muriatic acid is incapable of existing in a gaseous state without combined water: and I have stated facts, which appear to

me

me, to demonstrate, that, if muriatic acid gas does contain water, the proportion is not variable, but uniformly the same; that the smallest quantity cannot be abstracted without destroying the gas, and cannot be added without condensing it and forming liquid muriatic acid. In the given instance the hidrogen combines with the oximuriatic gas, forming common muriatic gas, a compound according to hypothesis of muriatic acid and water; there is no superfluous water produced, only the exact proportion necessary to render the acid gaseous: it is impossible, therefore, that the other part of the muriatic acid, which surrenders its oxigen to the gaseous oxide of carbon to form carbonic acid gas, can become gaseous; water is necessary for this change, and no water, but that confined in the muriatic acid gas already formed, is present. Yet it does become, according to the experiment, muriatic acid gas, which is a contradiction, and by itself amounts to a proof of the inaccuracy of the hypothesis.

Mr. Dalton adheres to the old system,

Mr. Dalton, in the second part of his "System of Chemical Philosophy," adheres to the old opinion of oximuriatic gas being a compound of muriatic gas and oxigen. In this respect he agrees with Mr. Murray; but his ideas concerning muriatic acid gas are very different from those of the latter gentleman. According to Mr. Dalton, muriatic acid gas itself is a simple body, containing no water. To account for the hidrogen produced when potassium and sodium are heated in this gas, he supposes, that these metals are compounds, that they consist of the fixed alkalis united respectively to hidrogen, which hidrogen is liberated, when muriate of potash and soda are made in the circumstances just mentioned. I cannot pretend to say, whether, when he adopted this hypothesis, he was aware of its consequences; whether he was acquainted with the fact, that there is no singularity attending the action of the metals of the alkalis on muriatic acid gas, and that tin, zinc, iron, and mercury, produce a similar separation of hidrogen from it. The necessary consequence of this view, if followed fairly, to its full extent, is the adoption of the doctrine of phlogiston. It is not my intention to discuss the merits of Mr. Dalton's speculations,

but his ideas of muriatic acid gas differ from Mr. Murray's.

His supposition

leads to the revival of the

I mention

phlogistic doctrine.

I mention them merely as they offer a contrast to Mr. Murray's. The instances of these two gentlemen show the nature of hypothetical reasoning, and its tendency, when followed, to draw the ablest philosophers into difficulties.

I am, Sir, with great respect,

Your very humble servant,

JOHN DAVY.

London, Feb. 9, 1811.

VI.

An Estimate of the Height, Velocity, and Magnitude of the Meteor, that exploded over Weston, in Connecticut, December the 14*th*, 1807: *with Methods of calculating Observations made on such Bodies. By* NATHANIEL BOWDITCH, *A. M. A. A. S. and Member of the Philosophical Society held at Philadelphia.*

(Concluded from p. 97.*)*

Observations at Wenham, Weston, and Rutland.

Account of the appearance of the meteor at Wenham.

SOME time after the appearance of the meteor, I went with Mr. Pickering to Mrs. Gardner's house in Wenham, where she had observed the phenomenon. She informed us, that on the morning of the fourteenth of December, 1807, when she rose, she went toward the window of her chamber, which looks to the westward, for the purpose of observing the weather, according to her invariable practice for many years past. The sky was clear, except a few thin clouds in the west. It was past day-break, and by estimation about half an hour before sunrise, or seven o'clock. The meteor was immediately observed just over the southern part of the barn in her farm yard, nearly in front of the window; its disc was well defined, and it resembled the moon so much, that, unprepared as Mrs. Gardner's mind was for a phenomenon of that nature, she was not at first aware, that it was not the moon, till she perceived it in motion, when her first reflection (to use her own words) was—where is the moon going to? The reflection however was hardly made, when she corrected herself, and with her eye followed the body with the closest attention throughout its whole course.

It

It moved in a direction nearly parallel to the horizon, and disappeared behind a cloud to the northward of the house of Samuel Blanchard, Esq. The true azimuth of the south part of the barn from the place of observation is N. 107° 59′ W. its altitude 3° 25′. The top part of the building is horizontal. The azimuth of Mr. Blanchard's house is N. 148° 22′ W. These buildings were useful in determining nearly the limits of the azimuths of the meteor.

Azimuths of the points of comparison.

The azimuths were obtained by observing with an excellent theodolite the difference of the azimuths of the sun and object, and finding the sun's azimuth by his observed altitude. The same method was made use of in obtaining the azimuths of the meteor. The angular elevation of the meteor above the horizon appeared always greater than that of the barn 3° 25′, and less than that of a tree in front of the window, along the branches of which the meteor ranged; the altitude of the top of this tree was 7° 10′. These fixed objects served to determine the altitude of the meteor, which is the most important element in the calculation, to a considerable degree of accuracy. After the theodolite was carefully adjusted, Mrs. Gardner directed the telescope attached to the instrument towards that part of the heavens where she first saw the meteor; the true azimuth was N 106° 54′ 54″ W. Altitude 5° 50′ 40″. The azimuth of a second place where it was seen was N 117° 35′ 54″ W. Altitude as before. The azimuth of a third place was N 132° 15′ 54″ W. Altitude 5° 29′ 40″. The azimuth of the place of disappearance was N 144° 33′ 54″ W. Altitude 4° 1′ 40″*. All these azimuths fall within the limits mentioned above, but the two last are undoubtedly too great. For a great circle passing through Wenham and Weston is inclined to the meridian of Wenham by an angle equal to 125° 18′ 38″, and as the two last azimuths exceed that quantity, they fall to the southward of Weston, which cannot be correct, because the meteor disappeared before it arrived at the zenith of Weston, as was observed by Judge Wheeler. It happens fortunately in the present

Method of obtaining them.

Elevation and azimuths of the meteor from these observations.

* In the following calculations the allowances made for refraction in the above observations, were 8′ 50″, 8′ 50″, 9′ and 10′ 10″ making the altitudes respectively 5° 41′ 50″, 5° 41′ 50″, 5° 20′ 40″, and 3° 51′ 50″.

instance,

instance, that the other places of observation at Weston and Rutland are so situate with respect to Wenham, that a considerable errour in the azimuths at Wenham would not materially affect the result of the calculation made for determining the height or direction of the meteor, as will appear in the following calculations. Mrs. Gardner supposed the meteor to have been visible about half a minute.

How long visible.

In its progress it was occasionally obscured by thin broken clouds, which intercepted the view of it several times. No

Velocity.

train of light was observed to accompany it. Its velocity did not appear to be so great as that of shooting stars. Its colour was more vivid than that of the moon. The place of observation at Wenham is in the latitude of 42° 40′ 15″ N, and in the longitude of 70° 50′ 15″ W from Greenwich.

Appearance of the meteor at Weston.

By the observations of Judge Wheeler at Weston, published in the interesting memoir of Professors Silliman and Kingsley, in the sixth volume of the Transactions of the American Philosophical Society held at Philadelphia, it appears, that on the fourteenth of December, 1807, at about 6h. 30′, A. M., "numerous spots of unclouded sky were "visible, and along the northern part of the horizon a space "of ten or fifteen degrees was perfectly clear. The attention of Judge Wheeler was first drawn by a sudden flash "of light, which illuminated every object. Looking up "he discovered in the north a globe of fire, just then passing behind the cloud, which obscured though it did not "entirely hide the meteor. In this situation its appearance was distinct, and well defined, like that of the sun "seen through a mist. It rose from the north, and proceeded in a direction nearly perpendicular to the horizon, but inclining, by a very small angle, to the west, "and deviating a little from the plane of a great circle, "but in pretty large curves, sometimes on one side of the "plane, and sometimes on the other, but never making an "angle with it of more than four or five degrees. Its apparent diameter was about one half or two thirds the apparent diameter of the full moon. Its progress was not "so rapid as that of common meteors and shooting stars. "When it passed behind the thinner clouds, it appeared "brighter

" brighter than before; and when it passed the spots of " clear sky, it flashed with a vivid light, yet not so intense " as the lightning of a thunder-storm. Where it was not " too much obscured by thick clouds, a waving conical " train of paler light was seen to attend it, in length about " 10 or 12 diameters of the body. In the clear sky a brisk " scintillation was observed about the body of the meteor, " like that of a burning fire-brand carried against the " wind. It disappeared about 15 degrees short of the ze- " nith, and about the same number of degrees west of the " meridian. It did not vanish instantaneously, but grew, " pretty rapidly, fainter and fainter, as a red-hot cannon " ball would do, if cooling in the dark, only with much " more rapidity.—The whole period between its first ap- " pearance and total extinction was estimated at about 30 " seconds. About 30 or 40 seconds after this, three loud " and distinct reports, like those of a four-pounder near " at hand, were heard. Then followed a rapid succession " of reports less loud—so as to produce a continued " rumbling. This noise continued about as long as the " body was in rising, and died away apparently in the di- " rection from which the meteor came."—Mr. Staples observed, " that when the meteor disappeared, there were " apparently three successive efforts or leaps of the fire " ball, which grew more dim at every throe, and disap- " peared with the last. From the various accounts which " we have received of the appearance of the body at differ- " ent places, we are inclined to believe, that the time be- " tween the disappearance and report, as estimated by " Judge Wheeler, is too little, and that a minute is the " least time that could have intervened." The latitude of Weston is about 41° 15′ N, longitude 73° 27′ W from Greenwich, but there is a little uncertainty in both these quantities, though not enough to affect materially the result of the calculation.

Appearance of the meteor at Rutland.

The observations made at Rutland were procured by the kind offices of Professor Hall, of Middlebury College, Vermont, to whom Mr. Page communicated his valuable observations in a paper, expressed in the following terms. " I was at the west door of my house on Monday morn- " ing,

" ing, the fourteenth of December, 1807, about day-light;
" and perceiving the sky suddenly illuminated, I raised my
" eyes, and beheld a meteor of a circular form, in the
" southwesterly part of the heavens, rapidly descending to
" the south, leaving behind it a vivid sparkling train of
" light. The atmosphere near the south part of the horizon
" was very hazy, but the passage of the meteor behind the
" clouds was visible, until it descended below the moun-
" tains, about twenty miles south of this place. There were
" white fleecy clouds scattered about the sky, but none so
" dense as to obscure the tract of the meteor. I now la-
" ment that I did not make more particular observations at
" the time, and I should probably until this day have con-
" sidered it to be what is commonly called a '*falling star*,'
" had I not read in the New York papers an account of the
" explosion of a meteor, and the falling of some meteoric
" stones near New Haven, Connecticut, which, by recur-
" ring to circumstances, then fresh in my recollection, I
" found to be on the same morning that I observed the
" meteor at Rutland. I am indebted to my learned friend,
" Dr. Samuel Williams, for his aid and directions in ascer-
" taining the situation of the meteor, when I first observed
" it, and its course, and also for the order of my observa-
Description and place of it.
" tions. *Form*, circular. *Magnitude*, less than a quarter
" of the diameter of the moon. *Colour*, red vivid light,
" *Tail*, or *train of light*, about eight times the length of its
" diameter at the least, projected opposite to its course.
" *Azimuth* when first observed, about 9° 30′ west of the
" meridian. *Altitude* when first observed, about 18° 30 *.
" Descent to the south part of the horizon, west of the me-
" ridian, by estimation 7 or 8 degrees. *Motion*, very rapid,
" probably thirty seconds in sight. *Place of observation*,
" Rutland, county of Rutland, and state of Vermont, la-
" titude 43° 36′ N, as ascertained by Dr. Williams; longi-
" tude west from Greenwich [72° 58′ 15″] as ascertained by
" Mr. Bowditch, by calculations made upon observation
" of the solar eclipse of June 16, 1806." In a letter,
which accompanied the preceding paper, Mr. Page ob-

* In making use of this altitude, 3′ for refraction was subtracted, making it 18° 27′.

served,

served, that his recollection of the meteor was pretty distinct, and that he was enabled to determine its situation by the position of certain known objects.

Deductions from the preceding observations.

Calculations from the preceding observations.

The observations made at Wenham, combined in various ways with those at Rutland and Weston, by the methods given in the preceding problems, furnish the results contained in Table 1, in which the given quantities are marked with an asterisk. These quantities are varied a few degrees in the different examples, for the purpose of forming an estimate of the change in the calculated place of the meteor, from any supposed errour in the observations. In the two first examples are combined (by Problem 2) the azimuth and altitude observed at the first appearance of the meteor at Rutland, with various supposed altitudes at Wenham. The 3d and 4th examples are like the first and second, except in a small change in the altitude and azimuth at Rutland. In the fifth and sixth examples, the first azimuth and altitude observed at Wenham are combined (by Problem 1) with the azimuth at Weston, supposing it in the 5th example to be north; and in the 6th N 3° W. The 7th and 8th examples contain the results from combining the same Wenham observation with the azimuth at Rutland, supposing it in the 7th example to be N 170° 30′ W, and in the 8th N 173° W. From the 9th to the 12th examples, the same calculations are repeated with the second Wenham azimuth and altitude. In the 13th example, the azimuth and altitude at Weston, at the time of the disappearance of the meteor, are combined with the altitude at Wenham, supposing it 5°: in the three following examples the calculation is repeated with small variations in the values of the given quantities. The azimuths at Wenham, calculated in the 13th and 14th examples, are made use of in the 17th, 18th, and 19th, with the corresponding assumed altitudes at Wenham, and the azimuth at Rutland at the time of the disappearance of the meteor, supposing it to be N 172° W in the 17th and 18th examples, and N 170° 30′ W in the 19th.

It appears by this table, that in the 5th and 6th examples

Calculations from the preceding observations.

altitude of the meteor at Weston was about 16° 30′, and in the 9th and 10th about 36′. Now if the meteor appeared at Weston to describe a great circle, beginning at the north part of the horizon and terminating at a point having the azimuth N 15° W and altitude 75° (which was nearly the case by the observations of Judge Wheeler) the azimuth corresponding to the altitudes 16° 30′ and 36′, would be respectively 1° 11′ and 2° 53′, as is easily found by spherics. The places of the meteor corresponding nearly to the azimuths 1° 11′ and 2° 53′, are given in the three right hand columns of the table opposite to those examples; these quantities being found by proportion. Thus by the 5th and 6th examples, the latitudes corresponding to the azimuths 0° and 3° are 42° 3′ 6″ and 42° 2′ 15″, varying 51″ by a change of 3° of azimuth; hence the latitude corresponding to the azimuth 1° 11′ is 42° 2′ 46″, or 42° 2′ 45″, as in the table. The other quantities were calculated in the same manner. The azimuth of the meteor at Wenham at the time of disappearing was not far from 123° 30′ or 124°, as is evident from examples 13, 14, 15, and 16. Mrs. Gardner supposed the meteor to have been visible considerably south of this point, which could not be correct for the reasons stated in the former part of this paper. By taking the second and third azimuths and altitudes of the meteor, as estimated by Mrs. Gardner, and finding by proportion the altitude corresponding to the azimuth 123° 30′ or 124°, and allowing 9′ or 10′ for refraction, the altitude will be found to differ but few minutes from 5° 30′, which is made use of in the 14th example. The estimated altitude would have been found nearly equal the same, if the four observations at Wenham had been taken into the calculation, by the usual method of interpolation, explained by Sir I. Newton in the Princip. Lib. 3, Lem 5*. The results of the 14th example are assumed in the right hand colums of Table I, and in Table II, as the true values at that time; and it is eiden t, that the latitudes and longitudes thus found must be nearly correct, since the meteor disappeared almost in the zenith of Weston, and a considerable errour in any of the

* In putting A for the azimuth at Wenham, and a for the corresponding altitude corrected for refraction, expressed in degrees and decimals, the formula of Newton gives $a = 5{\cdot}6972 - 0{\cdot}0009488 \cdot (A - 106{\cdot}90°)\,(A - 117{\cdot}60°) - 0{\cdot}0000312° \cdot (A - 106{\cdot}91°)\,(A - 117{\cdot}60°)\,(A - 132{\cdot}26°)$.

observations

observations would not materially affect the result, as appears by comparing examples 13, 14, 15, and 16. This latitude and longitude agrees nearly with that obtained in the 19th example, by combining the azimuth and altitude at Wenham with the azimuth 170° 30′ at Rutland; from which it appears probable, that the azimuth of the meteor at Rutland, at the time of its disappearance, must have been about 170° 30′, and the corresponding *calculated* altitude at that place 5° 45′*; but, by Mr. Page's observations, the azimuth of the meteor at the time of its disappearance was 172° or 173°, being about 2° greater than by the preceding calculations; and it seems reasonable to make the same allowance on the first azimuth, observed at Rutland. This correction being made, the azimuths become 168° 30′ and 170° 30′, and the altitudes 18° 27′ and 5° 45′, corresponding respectively to the first and last observations at Rutland. The azimuth correponding to any intermediate altitude may be found sufficiently near by supposing the variations of altitude and azimuth to be proportional to each other. In this way the azimuths corresponding to the altitudes of 6° 30′ and 7° 30′† would be respectively 170° 23′ and 170° 14′; which are rather less than the azimuths made use of in exampless 11 and 7. The changes to be made in the results of these examples, for this small difference of azimuth, may be easily estimated by comparing those examples with the 12th and 8th. In this way were found the assumed values, corresponding to those examples, given in the right hand columns of Table I. By taking the mean of the assumed values in Table I, corresponding to the examples 5, 6; and 7, 8; also the mean of those deduced from examples 9, 10; and 11, 12; there will be obtained the altitudes, latitudes, and longitudes of the meteor, marked in Table II, as the most probable values correponding to the times of the first and second observations made at Wenham.

Calculations from the preceding observations.

* Mr. Page states, that he saw the meteor till it descended below the mountains; but as it was hazy in the direction of the meteor, and the time early in the morning, it must have been difficult to determine by *observation* the precise point of its disappearance. The above method of *calculating* the altitude must give it very nearly correct.

† These are nearly the altitudes at Rutland at the time of the first two observations at Wenham, as appears by Examples 7, 8; 11, 12. An errour of several minutes in these altitudes would hardly cause any sensible change in the calculated azimuths at Rutland.

At

Calculations from the preceding observations.

At the first Rutland observation the meteor was not seen at Wenham, since the azimuth at Wenham at that time was probably less than 84° (as appears by the first four examples of Table I) and the first observed azimuth exceeded 106°; so that there can be no very accurate estimate of the situation of the meteor at that time. However, as the altitude of the meteor observed at Wenham, and its calculated altitude above the level of the sea (found in Table II), did not vary much during the time of its appearance at Wenham, it is highly probable that no change was experienced from the time of the first Rutland, to the first Wenham, observation. The preceding method of interpolation gives for the azimuth 83° 14′ 59″ at Wenham, a corresponding altitude at that place of 7° 43′*. This altitude with the azimuth at Rutland 168° 30′, and the corresponding altitude 18° 27′, give the results in Example 2, Table I; which, with an increase of 3′ 12″ in the longitude, are assumed in the right hand columns of that table and in Table II, as the place of the meteor at the first Rutland observation. This addition is made to the longitude, because the mean longitudes assumed in Table II exceed a few miles the results from the corresponding Rutland observations in Table I.

A mistake of 1° in the observed altitudes at Wenham would produce an errour of about $2\frac{1}{4}$ miles in the calculated height of the meteor; but the effect of this source of errour cannot be great, since the observed altitude must have fallen between the limits 3° 25′ and 7° 10′, corresponding to the heights of the barn and tree; as was observed above.

With the altitudes, latitudes, and longitudes of the meteor given in Table II, and the latitudes and longitudes of the places of observation, were calculated the distances of the meteor inserted in Table II, to give at one view the results of all the observations.

* This azimuth and altitude were found in the following manner. With the azimuth at Wenham, estimated in Example 1, 79° 33′ 46″, was calculated, by the preceding formula of interpolation, the corresponding altitude at Wenham. The difference between this and the supposed altitude 6° 30′ was called the errour of this supposition. The operation was repeated with another assumed altitude, as 8°, and the errour of this supposition found. By these errours a corrected altitude was calculated by the Rule of False; and by repeating the operation a few times, the above azimuth and altitude were obtained.

TABLE

TABLE I.

Places of the Meteor calculated with various Changes in the Values of the observed Angles for the purpose of estimating the Effect of supposed Errours in those Angles.

Exam.	Azimuths of the meteor at Wenham.	West.	Rutl.	Altitudes of the meteor at Wenh.	West.	Rutl.	Distances of the meteor from Wenh.	West.	Rutl.	Calculated places of the meteor. Alt.	Lat.	Long.	Assumed places of the meteor. Alt.	Lat.	Long
	° ′ ″		° ′	° ′	° ′	° ′	miles	miles	miles.	miles	° ′ ″	° ′ ″	miles	° ′	° ′
1st. Rutland Obs. 1	79 33 46		168½ *	6 30 *		18 27*	120·7		48·1	15·5	42 57 35	73 8 59			
2	83 14 59		168¾ *	7 43 *		18 27*	121·8		56·3	18·	42 51 2	73 10 48			
3	79 38 1		168 *	6 30 *		18 27*	121·2		48·2	15·5	42 57 31	73 9 29	18·2	42 51	73 14
4	77 46 52		168½ *	6 30 *		20 0*	120·6		44·5	15·4	43 0 43	73 8 7			
1st. Wenham Obs. 5	106 54 54*	0° *		5 41 50*	16° 11′		142·2	58·3		16·6	42 3 6	73 27 0			
6	106 54 54*	3		5 41 50*	16 50		145·2	57·6		17·0	42 2 15	73 30 19	16·7	42 2¼	73 28¼
7	106 54 54*		170½ *	5 41 50*		7 32	134·6		108·0	15·6	42 5 11	73 18 59			
8	106 54 54*		173 *	5 41 50*		7 22	129·3		105 6	14 9	42 6 35	73 13 13	15·7	42 5	73 19¼
2d Wenham Obs. 9	117 35 54*	0		5 41 50*	35 9		154·6	31 6		18·3	41 37 14	73 27 0			
10	117 35 54*	3 *		5 41 50*	36 14		156·1	31·2		18·5	41 36 35	73 28 31	18·5	41 37	73 28¼
11	117 35 54*		170½ *	5 41 50*		6 24	152·2		139·7	18·0	41 38 16	73 25 1			
12	117 35 54*		173 *	5 41 50*		6 14	144·6		135·1	16·9	41 41 23	73 17 23	18·1	41 38	73 25¼
Disappearance at Weston. 13	123 49 42	15 *		5 *	75 *		166·9	18·0		18·0	41 18 52	73 28 23			
14	123 38 51	15 *		5 30 *	75 *		166·9	20·2		19·5	41 19 20	73 28 33			
15	123 17 46	10 *		5 *	75 *		165·7	24·6		17·9	41 20 30	73 28 16	19 5	41 19½	73 28¼
16	124 17 56	15 *		5 *	80 *		167·4	18·4		18·1	41 17 39	73 27 57			
17	123 49 42*		172 *	5 *		5 08	161·6		158·5	17·3	41 21 34	73 23 52			
18	123 38 51*		172 *	5 30 *		5 40	161·3		158·0	18·7	41 22 9	73 23 46	19·5	41 19½	73 29¼
19	123 38 51*		170½ *	5 30 *		5 45	167·1		162·1	19·5	41 19 17	73 29 16			

TABLE II.

Places and Distances of the Meteor at the Times of the different Observations, as determined by the mean of all the Calculations.

Times of Observation.	Distances of the meteor from			Places of the meteor.		
	Wenh.	West.	Rutl.	Alt.	Lat.	Long.
	miles.	miles	miles.	miles.	North.	West.
At the 1st. Rutland observation.	124	113	57	18·2	42°51′	73 14′
At the 1st. Wenham observation.	159	59	110	16·2	42 04	73 24
At the 2d. Wenham observation.	154	82	141	18·3	41 37½	73 27
At the time of disappearing	167	20	162	19·5	41 19½	73 28½

Course of the meteor, and its height.

From the places of the meteor given in the preceding table it is easy to find, by the common rules of trigonometry, that *its course was about* S 7° W; *in a direction nearly parallel to the surface of the Earth, and at the height of about eighteen miles.* These points appear to be ascertained to a considerable degree of accuracy. The time elapsed between the disappearance of the meteor, and hearing the three loud reports at Weston, which, according to the estimates of different observers, was *at least* sixty seconds, serves in a degree to confirm the accuracy of the estimated altitude of the meteor. For the velocity of sound being 1142 feet per second, the distance corresponding to 60 seconds is 60 × 1142 = 68520 feet, or 13 miles nearly: consequently the height must have exceeded 13 miles.

Space passed over by it,

At the first appearance of the meteor at Rutland, it was elevated at least 8° above the horizon of Weston; and at its disappearance at Weston, was above 5° above the horizon of Rutland; as may be easily proved from the places of the meteor given in Table II. Now as it was seen by judge Wheeler and Mr. Page quite near the horizon, it must have been observed at both places from the time of the first Rutland observation till its disappearance at Weston. The distance of the points where the meteor was then situate is easily found from the data in Table II to be 107 miles in a straight line, and *the distance really passed over by the body while visible must have exceeded that quantity.* The whole duration of the appearance of the meteor, as estimated by Mr. Page and judge Wheeler, was about 30 seconds, which

which would make its velocity about 3½ miles per second, by both observations. In a similar manner the distance passed over, while visible at Wenham, was about 52½ miles, and if the duration of its appearance was 30 seconds, as Mrs. Gardner estimated it, the velocity corresponding would be 1¾ miles per second; this would have been more than doubled if the extreme azimuths at Wenham had been made use of without correction. From these results it appears probable that *the velocity of the meteor exceeded 3 miles per second.* We may form an idea of the greatness of this velocity, by observing that it is fourteen times as swift as the motion of sound, and nearly as great as that of a satellite revolving about the Earth at the same distance, and if a body were projected in a vertical direction with about double the velocity (the air being supposed not to resist) it would proceed beyond the sphere of the Earth's attraction. and its velocity.

In estimating the magnitude of the body from the present observations, no very accurate result is to be expected, since the apparent diameter was not exactly measured by any of the observers. The observations that were made serve however to prove, that the body was much larger than the whole mass of stones that fell near Weston, as will be evident from the following calculations. Mr. Page supposed the apparent diameter of the body to be about one quarter part of that of the moon, or about 8′. The greatest observed distance of the meteor from Rutland was 162 miles, the least 57 miles. The diameter of the meteor corresponding to those distances and the angle 8′ are nearly ¼ and ⅛ of a mile; and, by this observation, the real diameter of the meteor must fall between those limits. Judge Wheeler supposed the apparent diameter to be half or two thirds of that of the moon, or between 16′ and 24′. The least distance of the meteor from Weston was 20 miles, the greatest 113 miles. The least diameter corresponding to the distance of 20 miles and angle 16′ is $\frac{1}{11}$ of a mile, or rather 491 feet; the greatest diameter, corresponding to the distance 113 miles, and angle 24′ is nearly ¼ of a mile, so that the limits furnished by this observation are nearly $\frac{1}{11}$ and ¼ of a mile. Mrs. Gardner supposed the diameter to be Its magnitude.

equal

equal to that of the moon, or 32′; this, with the extreme distances at Wenham, 124 and 167 miles, furnish the limits $1\frac{1}{7}$ and $1\frac{1}{8}$ miles. This last estimate exceeds the others considerably; this may be owing in part to the smallness of the altitude of the object, which probably made it appear larger than it would otherwise have done, from the same cause which makes the moon appear largest when near the horizon. *The least of all the limits of the diameter of the meteor is* 491 *feet.* A body of this magnitude and of the same specific gravity as the stone that fell at Weston (which weighed about 225 pounds to a cubic foot) would contain a quantity of matter exceeding in weight *six millions of tons.* If the specific gravity were the same as that of the air at the surface of the Earth, the quantity of matter would exceed *two thousand tons:* and if the specific gravity were the same as that of the air at the height of the meteor (which by the usual rule for barometrical admeasurements is about $\frac{1}{15}$th part of that at the surface of the Earth) the quantity of matter would exceed *fifty tons.* Either of these estimates exceeds by far the weight of the whole mass that fell near Weston, which, by the accounts published, does not appear to have been greater than half a ton, and would not form a sphere of two feet diameter of the same specific gravity as the stone, as was observed by professor Day, in his valuable paper on the origin of meteoric stones. A sphere of this diameter, seen at the distance of the meteor from Wenham, would hardly be visible without the assistance of a telescope, since its apparent diameter would not exceed two thirds of a second. These reasons seem strongly to favour the opinion, that by far the greater part of the mass continued on its course without falling to the Earth, and the gradual disappearance of the meteor, as observed by judge Wheeler, is agreeable to this hypothesis.

Quantity of matter in it.

The whole apparently did not fall to the Earth.

As it is but within a few years, that observations of these meteors have been carefully made, we have not yet sufficient data for a well grounded theory of their nature and origin; none that has yet been proposed is free from difficulties. The greatness of the mass of the Weston meteor does not accord either with the supposition of its having been formed in our atmosphere, or projected from a volcano of the Earth

Earth or moon; and the striking uniformity of all the masses, that have fallen at different places and times (which indicates a common origin) does not, if we reason from the analogy of the planetary system, altogether agree with the supposition, that such bodies are satellites of the earth.

VII.

On Muriatic and Oximuriatic acid, in Reply to Mr. DALTON. *In a Leter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

MY object in "the remarks on the nature of potassium and sodium, in answer to Mr. Dalton," which you did me the honour to publish in your number for January last, was not so much to advance any thing *new*, as to state fairly the *arguments* used by Mr. Dalton to prove those bodies to be hidrurets; and the *facts*, which induced Mr. Davy to place them in the class of metals. It is not my intention to pursue that subject farther in this communication, as I feel satisfied, that even the splendid talents of Mr. Dalton, (should he honour my animadversions with a reply) will not be sufficient to controvert the deductions drawn by Mr. Davy from his experiments on these bodies—Indeed I should not have troubled you now, had not Mr. Dalton called on me, to explain my data, and mode of calculation, respecting the muriatic acid, and oximuriatic gasses. As to my *data being defective*, and the *principles* of *my calculations unintelligible* to Mr. Dalton, I can only say, that I have taken the former from Dr. Henry's Elements of Experimental Chemistry, 6th edition, 1810, (a work dedicated to Mr. Dalton) where in a table at p. 499, vol. II, I find the specific gravity of muriatic acid gas given as 1·430, according to Brisson—and ·929 according to Kirwan. Here is so great a difference, that, not knowing which to choose, I referred to

Remarks on potassium and sodium.

Principles of the writer's calculations.

Spec. grav. of muriatic acid gas.

to the article "Muriatic Acid" in vol. I, p. 448, where I find its weight stated with respect to common air, as 1·73 to 1·00—Considering this therefore as Dr. Henry's own opinion of its real weight, I assumed it as one basis for my calculation. I am aware that Mr. Davy, at p. 245, Phil. Trans. for 1810, states the weight of 100 cubic inches of muriatic acid gas, at 39 grs. which gives its specific gravity as 1·258—and from the acknowledged accuracy of that gentleman's experiments, I am inclined, on farther consideration, to adopt his estimate, which also nearly agrees with

Spec. grav. of oxigen gas.

that of Biot and Gay-Lussac. With respect to the specific gravity which I have assigned to oxigen [1·125] I acknowledge an errour I have inadvertently fallen into, from not remarking, that Mr. Davy's estimate of the weight of 100 cubic inches of that gas, viz. 35·06 (vide Henry's Elements, p. 178, vol. I,) is given at the temperature of 55°, and that consequently at 60° it would be but 34.7 (as stated indeed in the same paragraph), which gives its specific gravity, 1·119; and also I will farther allow, that (leaving 1·125 for oxigen) the number 90·72 should be 92·4. I must apologize to *you*, Sir, as well as to Mr. Dalton, for these inaccuracies; which arose from the necessity I was under of putting my thoughts to paper in greater haste than I should have done, had not other avocations claimed the time I would gladly have spent in their revisal. As to my mode of cal-

Mode of calculation.

culation, it is simply this:—Mr. Chenivix's estimate of the composition of oximuriatic gas, (viz. 77·5 muriatic acid +22·5, oxigen) is given by *weight*. To reduce these to proportionate quantities by measure, I divide the specific gravity of the *heavier* gas by that of the *lighter*. I multiply the *quantity* of the *lighter* by the quotient, and add the sum to the quantity of the heavier gas.—And I then say, as the sums so added, are to either of them, so is 100 to the proportional quantity required. Thus $\frac{1·73}{1·125} = 1·537 \times 22·5 = 34·5 + 77·5 = 112$. Then 112 : 34·5 :: 100 : 30·8 = the quantity of oxigen in 100 parts of oximuriatic gas, on the old hypothesis, by *measure*. Consequently this quantity would take twice its bulk of hidrogen, and the whole that would disappear on detonation, would be 30·8 ×

The corrected

3 = 92·4. If *these data* be true, my inaccuracies have been

been rather against my argument than for it; since the corrected numbers give a still larger diminution, than I had before asserted ought to take place. But taking the specific gravity of muriatic acid gas, after Mr. Davy, at only 1·258, and that of oxigen at 1·119, still the diminution ought to be greatly larger, than what really occurs, when 100 parts of oximuriatic, and an equal bulk of hidrogen gasses are detonated together, if the former contained oxigen. In this case, It should be in 200 parts 73·8, a proportion considerably more than ¼. This indeed approaches to what Mr. Dalton says he should have expected; but how can he reconcile even this difference with the real diminution that happens, and still support the old hypothesis? Whatever data be taken, I think the argument is decisive, in favour of Mr. Davy's views.

data more favourable to the argument.

I cannot close this communication without expressing my wish, that some philosopher, who has leisure, ability, industry, and *instruments* adapted to the labour, would make the *real weight* of the different gasses, in their *perfectly dry state*, the object of his investigations. It is a subject of material consequence, in many experiments of the refined nature that chemistry now happily begins to admit of, and most desirable, that they should be correctly ascertained; for as the case stands at present, specific gravities of these bodies may be found in our elementary works so widely differing from one another, that a man arguing for victory, rather than truth, may select data almost at will, as best happens to suit his purpose.—That this is not my case, I trust you and Mr. Dalton will readily believe: for I beg to assure that gentleman, that *truth only* is my object, and if he can bring forward *facts*, that prove his arguments correct, I shall be one of the first to acknowledge their justice, and to thank him for having rescued me from error.—I could not else again subscribe myself,

The real weights of the gasses still an important desideratum.

Your, and his humble servant,

JUSTUS.

Feb. 1811.

VIII.

On Cystic Oxide, a new Species of Urinary Calculus. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S.**

New species of stone in the bladder.

THE principal design of the present essay is to make known the existence, and to describe the leading properties of a new species of urinary calculus from the human bladder; but I shall at the same time take the opportunity of correcting an inaccuracy or two, that I have observed in my former communication on this subject. (Phil. Trans. 1797.)

Five kinds before described by the author.

I, on that occasion, took notice of five kinds of urinary calculi,

1. The *lithic acid*, since called *uric acid*, originally analysed by Scheele.
2. The *oxalate of lime*, or *mulberry calculus*.
3. The phosphate of lime, or *bone earth calculus*.
4. The ammoniacal phosphate of magnesia.
5. The *fusible calculus*, which consists of the two last species combined.

Another species since discovered.

It is now about five years since I first met with another species, evidently differing from each of those before described. It was in the possession of Dr. Reeve of Norwich, who obligingly gave me a portion of it for the purpose of examining its chemical qualities. It had been taken from his brother when he was five years old, and at that time was covered with a coating of phosphate of lime very loose in its texture, and consequently very soon separated†. This

Very rare.

species is probably very rare: for, although I have omitted no opportunity of paying attention to any urinary concretions to which I could have access, I have, to this time, seen

* Phil. Trans. for 1810, p. 223.

Stone with a hollow centre.

† I am informed, that another stone formed afterward in the bladder of this boy, and that he died in consequence, without submitting to the operation a second time. The stone found in his bladder after death consisted principally of uric acid, but was peculiar in one respect, as its centre was hollow by the removal of some more soluble substance, of which the nucleus had consisted.

only

only one other specimen of the same substance. This last is in a collection of calculi belonging to Guy's Hospital, given by Mr. Lucas, surgeon to that institution, having been formed partly by his father, and partly by himself, in the course of their practice; and according to the present arrangement, (which, it is to be hoped, will not be altered) the calculus to which I allude may be found by reference to No. 46 of that collection. It was extracted by the usual operation from a man of 36 years of age, of whom no record is preserved, except that his name was William Small. It weighed, when entire, 270 grains.

Description of it.

In appearance, these calculi resemble more nearly the triple phosphate of magnesia than any other calculus; but they are more compact than that compound is usually found to be: not consisting of distinct laminæ, but appearing as one mass confusedly crystallized throughout its substance. Hence, instead of having the opacity and whiteness observable in fusible calculi, which consist of a number of small crystals cemented together, these calculi have a yellowish semitransparency; and they have also a peculiar glistening lustre, like that of a body having a high refractive density.

Results of its distillation.

When this substance is submitted to destructive distillation, it yields fœtid carbonate of ammonia, partly fluid, and partly in a solid state, and a heavy fœtid oil, such as usually proceeds from animal substances; and there remains a black spongy coal, much smaller in proportion than is found after the distillation of uric calculi.

Action of the blowpipe.

Under the blow-pipe it may be distinguished from uric acid by the smell, which at no period resembles that of prussic acid; but in addition to the usual smell of burned animal substances; there is a peculiar fœtor, of which I cannot give a correct idea, as I know no smell which it can be said to resemble.

Peculiar smell.

Acted on by most chemical agents.

This species of calculus is so readily acted upon by the generality of common chemical agents, that its character may perhaps be most distinctly marked by an enumeration of those feeble powers, that it can resist.

Exceptions.

It is not dissolved (excepting in very small proportion) by water, by alcohol, by acetic acid, by tartaric acid, by citric acid, or by saturated carbonate of ammonia.

The

Solvents. Acid. The solvents on the contrary, are far more numerous. It is dissolved, in considerable quantity, by muriatic acid, by nitric acid, by sulphuric acid, by phosphoric acid, and by oxalic acid.

Alkaline. It is also dissolved readily by pure alkaline meustrua: by potash, by soda, by ammonia, and by lime water. It is even dissolved by fully saturated carbonates of potash or of soda. Accordingly, these alkalis are not so convenient for the precipitation of this matter from acid solutions, as the carbonate of ammonia, which is not capable of redissolving the precipitate, though added in excess.

Precipitates from acids and alkalis. For a similar reason, the acids best suited for its precipitation from alkaline solutions, are the acetic and citric acids. But the tartaric acid may occasion an appearance of precipitation, by forming a supertartrate with the alkali employed.

Forms crystals with acids, The combination of this substance with acids may be made to crystallize without difficulty; and they form slender spicula radiating from the centre, which readily dissolve again in water, unless they have been injured by being in any degree overheated.

The muriatic salt is decomposed by the heat of boiling water, on account of the volatility of the acid, and the rest are easily destroyed by a greater excess of heat.

The salt formed by combination with nitric acid does not yield oxalic acid, and does not become red, as the uric acid does, when similarly treated; but it turns brown, becoming gradually darker, till it is ultimately black.

and with alkalis. When the combinations with alkalis are evaporated, they leave small granular crystals; but as I was desirous of rendering my experiments as numerous as a limited quantity would permit, the portion which I could employ in any one experiment was too small for me to attempt to determine the form of such crystals.

Precipitated from potash by vinegar When a hot solution in potash was neutralized by distilled vinegar, the precipitate did not immediately take place, but formed gradually during the cooling of the liquor in minute crystals, some at the surface of the fluid, and others attached to the sides of the vessel. The only definite form, which I could observe, was that of flat hexagonal plates; but

In flat hexagonal crystals.

but I could discern nothing which enabled me to judge of the primitive form of the crystal. On the surface of the calculus belonging to Guy's Hospital, some minute crystals may be discerned of a different shape, being nearly cubic. And it is possible, that the hexagonal crystals may owe their figure to a small portion of alkali remaining in combination.

Perhaps cubic when pure.

From the ready disposition of this substance to unite with both acids and alkalis, it would appear to be an oxide; and that it does, in fact, contain oxigen, is proved by the formation of carbonic acid in distillation. The quantity of oxigen present in the calculus is not, however sufficient to give it acid properties, for it has no effect on paper coloured with litmus.

Combined with oxigen, but not an acid.

I am therefore inclined to consider it as an oxide; and since both the calculi, that have yet been observed, have been taken from the bladder, it may be convenient to give it the name of *cystic oxide*, which will serve to distinguish it from other calculi; and as this is unlike any other term at present employed in chemistry, it is to be hoped, that it will not be thought to require any alteration.

Cystic oxide.

Since the period of my first essay on gouty and urinary concretions, the general results contained in it have been confirmed by others, and I believe are incontrovertible. But I am under the necessity of acknowledging a mistake in the analysis of the mulberry calculus, though not of much importance. An acid is mentioned to have arisen by sublimation, and it was supposed to originate from a partial decomposition of the oxalic acid. But since pure oxalate of lime yields no such sublimate, it most probably arose from a mixture of the small quantity of uric acid in the calculus then under examination.

Mulberry calculus.

In the analysis of the triple phosphate of magnesia, there is another mistake of more consequence. In my selection from numerous experiments for ascertaining the presence of phosphoric acid, I gave the preference to one in which nitrate of mercury was employed, on account of the facility of extracting the acid from the phosphate of mercury by heat alone. But since the whole of the phosphoric acid is not precipitated by nitrate of mercury, sulphate of magnesia

Mistake in the analysis of the triple phosphate of magnesia.

will not be formed on the addition of sulphuric acid, the magnesia cannot be obtained separate by the same process.

led to another. It may have been in consequence of this oversight, that mistake on that subject has occurred in the succeeding volume of the Transactions.

Calculus of a dog supposed to contain lime

A calculus is there described, which had been taken by Mr. Thomas from the bladder of a dog; and a series of experiments are related, from which it was inferred to consist of superphosphate of lime, and phosphate of ammonia. But from the appearance of this calculus (which was exhibited to the society at the time when the paper was read) I was much inclined to think, that the nature of it was mistaken; and upon full consideration of the experiments, they did not appear to me conclusive.

was afterward proved to be a compound of magnesia.

I therefore obtained a portion of the calculus, and, by the following process, the earth contained in it was proved to consist almost wholly of magnesia.

It was dissolved, with the exception of a very small residuum, by distilled vinegar.

The whole of the phosphoric acid was then precipitated by acetate of lead added to excess.

The liquor was then poured off, and sulphuric acid was added, which precipitated the excess of lead, and at the same time formed sulphate of magnesia in solution.

By evaporation to dryness, the acetic acid was removed, and by subsequent increase of heat, the sulphate of ammonia, and excess of sulphuric acid were expelled.

The residuum being then dissolved in water, and the liquor suffered to crystallize by spontaneous evaporation, there remained a quantity of sulphate of magnesia, that weighed rather more than the quantity of calculus taken for the experiment.

Superphosphate of lime perhaps never exists in a calculus.

It was evident, therefore, that in this instance the calculus examined did not consist of superphosphate of lime; and there is some reason to doubt, whether a compound, that is so very soluble in water, ever forms a part of urinary concretions.

Prevention of [illegible]

Although the treatment of diseases is not in general a fit subject to occupy the time of this society, there is nevertheless

theless one suggestion, with respect to the prevention of calculous complaints, so nearly connected with my present subject, that I think it may deserve to be recorded.

Since the white matter contained in the urine of birds, which is voided along with their dung, has been remarked by Mr. Vauquelin, to consist principally of uric acid, I have paid some attention to the different proportion in which this matter is voided by different species of birds, to see how far it accorded with the different qualities of their food. And I found, that in the dung of the goose, feeding wholly on grass, the proportion did not seem so much as $\frac{1}{100}$ of the whole dung. In that of a pheasant kept in a cage, and fed on barley alone, it was about $\frac{1}{17}$ part. In that of a hen, having the range of a garden and farm yard, and consequently procuring insects, and possibly other animal food, the proportion was manifestly much greater, and combined with lime. In the dung of a hawk, fed upon flesh alone, the quantity of matter voided in a solid state bears but a small proportion to the residuum of uric acid, that is left by the urine when dry. And in the gannet, feeding solely on fish, I have observed the evacuations in some instances to be mere urine, for it contained no solid matter excepting the uric acid.

Uric acid in the dung of birds

seems proportional to the quantity of animal food they take,

particularly of fish.

It seems, consequently, deserving of inquiry, what changes might be produced in the urine of any one animal, by such alterations of diet, as its constitution would permit; for as far as any inference can be drawn from these varieties, which naturally occur, it would appear, that persons subject to calculi, consisting of uric acid, as well as gouty persons, in whom there is a redundance of the same matter, have much reason to prefer vegetable diet: but that the preference usually given to fish above other kinds of animal food is probably erroneous.

Hence calculous and gouty persons should prefer a vegetable diet.

IX.

Comparative Analysis of Gum Resins: by Mr. HENRY BRACONNOT, *Professor of Natural History, &c.*

(Continued from vol. XXVII, *p.* 370.*)*

ART. II. *Analysis of Gamboge.*

Effects of heat on gamboge.

Sect. I. IF gamboge be exposed to the flame of a candle, it swells up; and burns like a resin. Heated in a capsule it emits a peculiar smell, softens, and is decomposed before it melts.

Products of its distillation.

Fifty gram. [772 grs.] exposed to distillation produced

1st, a brown water, containing empyreumatic acetic acid:

2d, a small quantity of a light oil:

3d, afterward came over in considerable quantity another oil, heavy, thick, and brown.

Coal.

In the retort remained a light coal, weighing 8 gram. [123·5 grs]. It was incinerated with difficulty, and left 5 decig. [7·7 grs] of ashes, which yielded 2 cent. [0·3 of a gr.[of potash in part sulphated, 4 cent. [0·6 of a gr.] of phosphate of lime, 6 cent. [0·9 of a gr.] of carbonate of lime, and 3 dec. [4·6 grs] of quartz sand, containing a little charcoal, and some traces of oxide of iron. No ammonia was found in the liquid products.

Gamboge treated with alcohol,

Sect. II. Twenty gram. [309 grs] were treated with hot alcohol and filtered. What remained on the filter, after being well washed with alcohol, was a grayish substance, that dried with difficulty, and was then brittle. In this state it weighed exactly 4 gram. [61·7 grs]; had a faint taste, or was nearly insipid; and dissolved entirely in water, except a decig. [1·5 gr.] of impurities. The solution reddened litmus. Evaporated to dryness it left a transparent, friable residuum, resembling the high coloured gum of the plum-tree, burning like it with little flame, and leaving a considerable quantity of a compact coal, in which was some phosphate of lime.

The alcoholic solution was red. Evaporated to dryness it

it afforded 16 gram. [247 grs] of resin. This was transparent, red, without any perceptible taste, and pretty decidedly electric by friction. When powdered it emitted a peculiar smell, and assumed a bright yellow colour.

On pouring water into a saturated solution of this resin in alcohol, there is a sensible evolution of heat, and a uniform, yellowish, milky liquid is produced; while most other resins are precipitated from alcohol in clots. precipitated by water,

Sect. III. Solution of potash acts very quickly on the resin of gamboge, particularly if heated. The result is a red liquid of an oily appearance, in which the properties of the potash are neutralized. On evaporating this compound almost to dryness, it crystallizes like the solution of aloes. and acted on by potash.

The soap or saponule of this resin is of a deep red approaching to black, and feels greasy between the fingers. When dried it is friable, and resembles a resin. It has the taste of rancid fat, leaving a slight sensation of acrimony at the root of the tongue. It is easily soluble in water, without rendering it turbid. Acids precipitate this solution in such abundance, that the whole becomes a thick coagulum of a fine yellow colour. Lime-water throws down from it a fine orange coloured precipitate. Earthy salts and most solutions of the white metals likewise produce yellow precipitates in it. It precipitates sulphate of iron brown, and nitrate of copper green. Soap from it.

Sect. IV. Ten gram. [154·4 grs] of the resin of gamboge were put into a retort with 80 parts of strong nitric acid of the shops. As soon as the retort felt the heat of the fire, red vapours arose, the intensity of which soon disappeared. The first product was returned into the retort, and the operation continued, till the matter was dissolved, and the solution reduced to the consistence of a sirup*. On cooling a mass of lamellar crystals was formed, enveloped in a vis- The resin treated with nitric acid.

* The product of this distillation, being thoroughly saturated with chalk, and distilled anew, yielded a slightly acid liquor, of a very pungent smell, and bitter to the taste. Alkali gave it a light yellow tinge. On adding sulphate of iron to this mixture a precipitate was formed, which was completely soluble in acids; so that there was no prussic acid in this liquid, but I am not fully acquainted with its nature. Product of the distillation.

cid

cid matter. The whole being diluted with a quantity of water, a sediment was formed, which, when well washed and dried, weighed 1·3 gram. [20 grs].

Product. This matter is of a yellowish colour, and bitter taste. It is partly soluble in boiling water. On cooling the solution grows turbid, and lets fall a portion. The filtered solution is of a reddish yellow, froths when shaken, reddens infusion of litmus, is rendered of a deeper colour by the addition of an alkali, and forms a slight precipitate after some time with sulphate of iron.

On burning coals this substance does not melt so easily as the resin, diffuses a fragrant smell, and leaves a great deal of coal.

It combines very well with potash and spirit of wine, forming with them transparent red solutions.

Nitric acid, heated gently with it, dissolve it, without occasioning any sensible alteration. Water produces a copious white coagulum in the solution.

A resinoamer. From these properties I think I may fairly consider this substance as a particular species of soluble factitious resinoamer, combined with a yellowish resiniform substance insoluble in water.

Produce of the mother waters. The acid liquor and waters of elutriation were boiled down, to drive off any nitric acid that might remain; and the residuum was diluted with water, in which a small quantity of potash was dissolved, which separated 4 dec. [6 grs] of the yellow resiniform substance. The liquor being again boiled down, then treated with alcohol, and filtered, left 1 gram. [15·4 grs] of very white superoxalate of potash. The alcoholic solution produced on evaporation 3 gram. [46·3 grs] of bitter matter, soluble in water, and containing malic acid.

The resin acted on by oxi-muriatic gas *Sect.* V. I diluted some of the resin of gamboge in fine powder with water; and passed into it a stream of oximuriatic acid gas, to try its effect on the colour; and in fact it destroyed its fine yellow. The milky liquor being boiled down, then diluted with water, and filtered, left on the filter a substance, which was washed with boiling water, till what came off would no longer redden litmus. The following were its properties.

It

It is pulverulent, of a pale yellow colour, and without any perceptible taste. It crackles between the teeth like an insoluble salt, and boiling water will not dissolve it. It is very little fusible, and emits no smell, till it begins to be decomposed; but if it be set on fire, or thrown on burning coals, it emits pungent fumes of muriatic acid. Properties of the product.

Weak acids separate nothing perceptible from it; but with concentrated acids charcoal and muriatic acid are produced.

Combined with potash the compound has a pleasant soapy smell, and nitrate of silver throws down from its solution a precipitate partly soluble in nitric acid.

I distilled 6 gram. [92·6 grs] of this substance in a small retort, which was heated to redness. The product was collected in a few decigr. [about ¼ of an oz measure each] of water, which, being examined toward the end of the distillation, was very sour, and had the smell of muriatic acid. To this water I added nitrate of silver, which produced a copious curdy precipitate of muriate of silver. This precipitate weighed 5·4 gr. [83·4 grs], which would contain 1·35 gram. [20·84 grs] of muriatic acid, according to the proportions given by Bergman of 25 acid to 75 oxide*. Distilled.

In the retort were left 2·1 gram. [32·4 grs] of a tumid coal.

Hence it follows, that 100 parts of this acidiferous resinous substance were composed of

Dry muriatic acid	22·5
Charcoal	35
Oxigen, hidrogen, and carbon	42·5
	100

Its component parts.

Sect. VI. Thus it appears, that gamboge is a true gum-resin, since we find in it a peculiar resin very well marked, and a gum resembling that of many of our fruit trees. Gamboge a true gum-resin.

* This certainly estimates the acid too high. From 19 to 19·5 per cent of acid is the most that can be allowed, according to the experiments of several of the most eminent modern chemists. C.

ART.

Art. III. *Analysis of Euphorbium.*

Euphorbium from different plants.

Sect. I. The Dutch have their euphorbium from Malabar, where it flows from the euphorbia antiquorum; that used in England is from the euphorbia canariensis*, which furnishes the euphorbium used in France, as Mr. Braconnot had an opportunity of satisfying himself, by finding several branches of the tree among the specimens he examined.

Action of heat on it.

Sect. II. Euphorbium, when exposed to a gentle heat, softens easily, and loses a twentieth of its weight in moisture.

Boiled in water.

I boiled 4 gram. [61·8 grs] in 100 gram. [1544 grs] of distilled water. The filtered liquor left an insoluble substance, which, when dried, weighed 3 gram. [46·3 grs.]. What passed through was of an amber colour, and had a bitter taste with a slight degree of acrimony.

Properties of the solution.

This solution reddened infusion of litmus. Oxalate of potash threw down from it a pretty copious precipitate of oxalate of lime. Nitrate of lead formed in it a white precipitate entirely soluble in distilled vinegar. Lime-water rendered it turbid, and occasioned a yellow precipitate, which vinegar dissolved.

Boiled in alcohol.

Sect. III. *A.* I boiled 20 gram. [308·8 grs] of euphorbium in 90 gram. [1389·6 grs] of alcohol at 36° [spec. grav. 0·837], which was sufficient to dissolve all the parts capable of solution. This solution filtered at a boiling heat left a substance on the filter, which, after being well washed with alcohol and dried, weighed 6·4 gram. [98·8 grs.]

Deposit from the alcohol on standing,

B. Having mixed together the alcoholic solutions, which had grown turbid on cooling, and let them stand at rest for two days, a considerable quantity of a white, granular, and somewhat gelatinous substance was deposited, which, being washed with alcohol and dried, weighed 4·7 gram. [72·5 grs]. It still retained some alcohol, which being driven off by heat, only 3·4 gram. [52·5 grs] remained.

similar to wax.

This substance was semitransparent, capable of being indented by a hard body, softened readily between the fingers, was almost wholly volitalized on a redhot iron, and com-

* According to our college, who have readmitted it into their last Pharmacopœia, from the euphorbia officinarum. C.

ported

ported itself like bees wax, which it resembles in smell when melted or burned. This wax of euphorbium retained a slight degree of acrimony, no doubt because it had not been sufficiently washed with alcohol. I made a taper of it, which burned with a very clear flame.

The matter insoluble in alcohol boiled in water

C. The 6·4 gram. [98·8 grs] insoluble in alcohol (*A*) were heated to the boiling point in 100 gram. [1544 grs] of distilled water. The filtered liquor left behind little bits of wood and thorns, which when dried weighed 2·7 dec. [4·17 grs].

and evaporated,

D. The aqueous solution (*C*), being evaporated, formed a varnish on the surface of the glass. On evaporating to dryness a brittle substance was obtained, which separated in micaceous scales, did not attract moisture, and weighed 4·1 gram. [63.3 grs], which I perceived at once to be malate of lime*. In fact, on heating this substance with diluted sulphuric acid, I obtained, 1st, very white sulphate of lime, which, after being washed and dried, weighed 1·6 gram. [24·7 grs]: 2dly, an acid, which alcohol dissolved, and from which it separated 5 dec. [7 grs] of sulphate of lime. The solution, being evaporated, produced 2 gram. [30·83 grs.] of malic acid, retaining a little sulphuric acid, which was separated by barytes.

yielded malate of lime.

This mistaken for gum and extract.

Malate of lime therefore appears to exist in tolerable quantity in the milky juice of the euphorbias; and it was this salt, which the older chemists mistook for a gum in the euphorbium of the shops, and which Mr. Laudet has confounded with extract.

More wax obtained.

E. The alcoholic solution (*B*), being evaporated to dryness, left a residuum weighing 8·3 gram. [128 grs]. This was treated with cold alcohol, which dissolved the resinous parts, and farther separated 4 dec. [6·2 grs.] of wax.

Resins mixed with malate of potash.

F. This solution being evaporated afresh, a resin was obtained, that attracted moisture from the air in a small degree. This was owing to the presence of malate of potash, which I obtained by heating the resin with distilled water. When well dried it weighed 4 dec. [6·2 grs.]

* I have already made known, that the ricinus, which is of the family of euphorbias, contains malic acid, neutralizing a large quantity of potash and lime.

G. The

Properties of the resin.

G. The resin of euphorbium has a reddish transparency, and extreme acrimony, whence it may be considered as a violent poison. It becomes electric by friction. Alkalis have no sensible action on it. Sulphuric acid dissolves it cold. Treated with nitric acid at a heat of 20° [77° F.] it softens, grows yellow, and begins to decompose. By increasing the heat, a complete solution is obtained, which, when evaporated, yielded a great deal of yellowish resiniform matter, a soluble resinoamer substance, and some traces of oxalic acid.

Component parts of euphorbium.

Sect. IV. From these experiments it follows, that 100 parts of euphorbium are composed of

Water	5
Wax	19
Woody matter	13·5
Malate of lime	20.5
——— potash	
Resin	37
Loss	3
	100

ART. IV.—*Analysis of Myrrh.*

Myrrh distilled alone.

Sect. I. Thirty gram. [463·2 grs.] of myrrh gradually heated to redness in a retort yielded 10 gram. [154.4 grs.] of an empyreumatic, heavy, brown oil; 10 gram. [154·4 grs.] of a red liquid, that changed sirup of violets green, yielded ammonia when mixed with potash, and acetate of potash mixed with oil when evaporated.

Coal.

The coal left in the retort occupied less space than the myrrh itself, was compact, iridescent, shining, and weighed 7·25 gram. [112 grs.] Being icinerated it produced 1·6 gram. [24·7 grs.] of white ashes, which furnished by elutriation 7 cent. [1.08 grs.] of sulphate of potash mixed with a small quantity of subcarbonate.

The part of the ashes insoluble in water dissolved completely in nitric acid with effervescence, carbonic acid being evolved mixed with a little sulphuretted hidrogen, arising no doubt from the decomposition of a small quantity of sulphate of potash by the lime and carbon. The nitric solution

lution contained nothing but lime, which was separated entirely by subcarbonate of potash.

Sect. II. *A.* Fifty gram. [772 grs.] of myrrh distilled with water yielded a product having the smell of the myrrh, and in which floated a little volatile oil. Myrrh distilled with water.

B. The residuum of this distillation being thrown on a filter, the liquid was a long time in passing through. The matter that did not dissolve was well washed with boiling water.

C. The several liquors, being mixed and evaporated to dryness, left 23 gram. [355 grs.] of a red transparent gum, bitter to the taste. Gum.

1. This gum reddened infusion of litmus. Its properties.

2. Treated with boiling water it dissolved only in part. What remained was a substance of a gummy appearance, and was perfectly insoluble even in dilute acids. It was bulky, became brittle when dried, and afterwards swelled up in boiling water without dissolving. This insolubility acquired by the gum of myrrh appears to be owing to heat.

3. The gum of myrrh yielded by distillation subacetate of ammonia, some oil, and a coal, which incinerates very easily compared with those of animal matters.

4. Treated with dilute nitric acid, and gently heated, it gave out carbonic acid gas mixed with nitrogen; a yellow flocculent matter was deposited, which soon disappeared; and, the liquor being evaporated, oxalic acid remained, mixed with malic acid, and a bitter yellow matter, which did not detonate.

5. Oxalate of potash threw down oxalate of lime from the solution of the gum.

6. Lime-water in excess did not alter its transparency, so that it contained neither malic nor phosphoric acid.

7. Neither decoction of galls nor oximuriatic acid occasioned any change in it.

8. Several metallic solutions, as those of lead, mercury, and tin, form copious white precipitates in the aqueous solution of this gum.

The precipitate produced by nitrate of lead in 23 gram. [355 grs.] of this gum dissolved in water weighed 20·2 gram. 311·8 grs.]. It was of a reddish colour, like myrrh, and semitransparent

semitransparent. Boiling water only divided it; but, on adding gradually weak sulphuric acid to a very slight excess, the clots disappeared, and it was filtered. What remained on the filter, when well washed and dried, consisted of 5·3 gram. [81·8 grs.] of sulphate of lead, containing 4 gram. [61·76 grs.] of oxide; which had been combined with 16 gram. [247 grs.] of the gum, that were reobtained by evaporating the liquor separated from the sulphate of lead. This gum, which retained an excess of acid, yielded neither malic nor phosphoric acid when treated with alcohol; which led me to suspect, that the lime contained in the gum of myrrh is saturated by acetic acid; and perhaps also by carbonic; for, if sulphuric acid be poured into a solution of this gum, a slight effervescence takes place, and a precipitate of sulphate of lime.

We see from what precedes, that the gum of myrrh is not entirely separated from its solvent by the nitrate of lead; since of the 23 gram. [355 grs.] only 16 [247 grs.] fell down with the oxide. This must have been owing to the nitric acid set free; for a salt of lead surcharged with oxide, as the subacetate, precipitates almost wholly the solution of the gum of myrrh, and entirely, if a little alkali be added to the mixture.

The part insoluble in water treated with alcohol.

D. What remained on the filter (*B.*) was heated with alcohol, which dissolved all the resinous parts, and left behind a soft, transparent, substance, insoluble in boiling water, and weighing when dried 6 gram. [92.6 grs.]. It had all the properties of the gummy matter already mentioned.

Resin produced.

E. The alcoholic solution (*D.*), mixed with the spirit used in washing the filters, yielded on evaporation 11·5 gram. [177·6 grs.] of a brown resin, with an aromatic bitter taste like that of myrrh.

Its properties.

1. This resin softens easily between the fingers, melts at 48° R. [140° F.], and does not become electric.

2. It emits an aromatic smoke while burning, and yields on distillation the same products as resins.

3. With potash it forms a kind of soap, the aqueous solution of which passes turbid through the filter.

4. On 5·5 gram. [85 grs.] of this resin of myrrh were poured

poured 33 gram. [509.5 grs.] of nitric acid at 38°, which turned the resin blackish. The mixture being distilled emitted red vapours, but not very abundantly. After having obtained a product of about 20 gram. [309 grs.] from this solution, the retort was removed from the fire. A resiniform substance of an orange colour floated in it, which, when washed and dried was of a pale yellow, and weighed 1·5 gram. [23 grs.]. It was pulverulent, bitter, not very fusible, partly soluble in water, and formed with potash a saponaceous compound, which dissolved very easily in water, giving it a red colour, without any diminution of its transparency. This resiniform substance contains a great deal of carbon, and is sensibly altered by nitric acid, which only dissolves it. In other respects it comports itself like the resin of gamboge treated with nitric acid.

The nitric solution, on which this substance floated, being evaporated to dryness, left a residuum, which being well washed, furnished 1 gram. [15·4 grs.] more of the resiniform substance, which had been kept dissolved by means of the nitric acid. Lime-water in excess added to the waters of elutriation separated 1·2 gram. [18·5 grs.] of oxalate of lime mingled with a small portion of malate. The supernatant liquid contained a bitter yellow matter.

Sect. III. From these experiments it follows, that myrrh consists principally of a gum different from the common kind, the leading properties of which are

Myrrh consists chiefly of a gum.

1, To acquire a degree of cohesion from the action of heat, when its solutions are evaporated, which renders it partly insoluble in water:

Its chief characteristics.

2, To give out ammonia when distilled, and nitrogen gas when acted on by nitric acid; which gives it an affinity to animal substances:

3, To decompose solutions of lead, mercury, and tin, and unite with their oxides.

Myrrh contains likewise about 0·23 of its weight of a very fusible bitter resinous matter.

(To be continued.)

SCIENTIFIC

SCIENTIFIC NEWS.

Royal Society of Edinburgh.

Royal Society of Edinburgh.

ON the 7th of January Sir George M'Kenzie continued his account of the Mineralogy of Iceland, and described some very curious geological facts.

On the 21st he concluded his mineralogical detail, with an interesting description of Mount Hecla and other volcanic districts. In this paper Sir George made some remarks which tended to place obsidian and pumice in a conspicuous point of view, as relating to the different theories of the Earth, and clearly proved their origin to be igneous, a position which has hitherto been denied by Werner and his pupils.

On the 4th of February Dr. Brewster read an ingenious paper on the loss of the comet of 1770.—Sir George M'Kenzie described some remarkable hot springs in Iceland. To one of these he gave the name of the alternating *Geyser*, as it spouted from two distinct orifices evidently connected within, but only from one at a time, the operations of which alternated with those of the other at regular intervals of time.

On the 18th Professor Playfair read part of a Biographical Memoir of the late John Robison, LL.D. and Professor of Natural Philosophy in the University of Edinburgh. Mr. Allan communicated a letter from Dr. Henry, of Manchester, describing the position of some singular masses of a substance apparently composed of wax and resin, which had been laid bare by a late overflow of the river Mersey, a little below Stockport, about three feet under the soil, and supposed to be the refuse of some manufactory, of which no other vestige or recollection now remains.

Geological Society.

The First Volume of the Transactions of the Geological Society, in 4to, with many plates, is in the press, and will be ready for publication in the month of May next.

TO

TO CORRESPONDENTS.

Philochemicus does not seem to be aware, that the object of the galvanic troughs he mentions was to get rid of the cement, all the disadvantages of which recur in the plan he proposes. Any danger to be apprehended from the use of earthenware or glass may readily be obviated by the simple and cheap defence of a case of common wood.

With regard to the use of the word particle, when he defines it, in one place, the smallest portion of any compound substance into which it can be resolved without decomposition; and, in another, the smallest portion of any compound body, which we can procure by mechanical means; these two definitions are obviously not consistent with philosophical precision. The fact appears to be, that the word particle, or small part, a word with a certain laxity of signification in common use, cannot well therefore be confined to one precise and definite meaning on all occasions; but like many other terms, will have its exact sense to be determined in general by the context.

L. O. C.'s paper will be inserted the earliest opportunity.

As a request formerly made appears not to have been seen, or to have been overlooked, by some of my more recent correspondents, I beg leave to repeat it. It is, that they would leave in their manuscript a margin for the insertion of the side notes. It is also desirable, that, when a paper requires drawings for its illustration, none of it should be written on the back of the drawings.

METEOROLOGICAL

METEOROLOGICAL JOURNAL,

For FEBRUARY, 1811,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JAN. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
28	30·5	30·5	33°	24°	29·61		Fair	Fair
29	27	29	31·5	23	·60		Ditto	Ditto
30	27	33·5	33·5	33.5	·73		Ditto	Cloudy*
31	37	42	43·5	33	·16		Rain	Rain
FEB.								
1	39	40	42·5	35·5	·22	·810	Ditto	Ditto
2	40	33	44·5	40	·62	·145	Fair	Fair
3	45	40	47	33	·33		Ditto	Ditto
4	36·5	37	44	34	30·08		Ditto	Ditto
5	37·5	41	42	38	·05		Cloudy	Ditto
6	44	46	47	42	29·61		Ditto	Ditto
7	45	44	47	39	·60		Rain	Ditto
8	44	47	48·5	39	·70	·675	Ditto	Cloudy
9	42	45	49	41	·60	·015	Ditto	Ditto
10	46·5	50	51	41·5	·82	·020	Ditto	Rain
11	48	49	50·5	38	·54	·030	Ditto	Ditto
12	48	46	49	34	·30	·100	Ditto	Fair
13	38	38	40	33	·32	·010	Ditto	Ditto
14	39	40	42	34	·26	·050	Fair	Foggy
15	37	40	41·5	34	·68		Rain	Rain
16	39	35	41	28	·44	·210	Fair	Fair
17	32	36	37	33	30·24		Cloudy	Ditto
18	37	38	41·5	31·5	·13		Fair	Ditto
19	35	35	39·5	32	29·97		Ditto	Ditto
20	36	40	40	36	·73		Ditto	Foggy
21	40	44·5	44·5	40	·47		Rain	Rain
22	44	42	47	38	28·97	·060	Ditto	Fair
23	42	44	46·5	40	29·20	·060	Ditto	Cloudy
24	45	44·5	47·5	36	·13	·190	Ditto	Rain
25	41	44	48	40	·26	·080	Fair	Fair†
26	49	46	52	40	·21	·180	Rain	Ditto

2·645 Inch. since last Journ.

* Heavy snow in the night and Thaw.

† Rain in the Night.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1811.

ARTICLE I.

Observations and Experiments on the Alkaline Metalloids: by J. MURRAY, *Lecturer on Chemistry, &c., Edinburgh.*

To Mr. NICHOLSON.

SIR,

THE speculation with regard to the nature of the metallic bases of the alkalis, which I venture to submit to your readers in the following pages, occurred to me some months ago, on perusing a paper by Berthollet in the 2d volume of the Mémoires D'Arcueil, on the existence of combined water in potash. Having only lately had it in my power to perform the experiments connected with the investigation, I now transmit to you a general account of the results, which may not perhaps be without interest in the present state of chemical inquiries.

Berthollet's remarks on water in potash,

In the memoir to which I allude, Berthollet relates experiments, which appear to prove, that potash prepared by the usual process contains a considerable proportion of water, from which, while it exists in an insulated state, it cannot be freed by the application of heat. The heat at

which cannot be expelled by heat.

first dissipates a portion of the water; but in continuing the application of it, the potash itself rises in vapour before the whole of the water is expelled. Hence Berthollet found, that potash in this state yields, when it is mixed with iron filings, and exposed to a sufficiently elevated temperature, a large quantity of hidrogen gas. From this it appears evidently to contain water; and from the results of its combination with muriatic acid he infers, that the quantity amounts to 13·64 in 100 parts of potash which has been kept in fusion until it rises in vapour.

This apparently incompatible with the theory of the decomposition of potash by galvanism,

Admitting this, it appears to lead to conclusions incompatible with the theory, which has usually been received, of the decomposition of potash by galvanism. In this decomposition oxigen is given out at the positive side, and potassium appears at the negative side; and according to the theory given by Mr. Davy of this experiment, this is owing to the decomposition of the alkali—the potassium is its base, and the oxigen the principle, with which this base had been combined.

as the water should be decomposed, but no hidrogen is evolved.

But if the existence of water in potash be admitted, the results are not conformable to this theory. This water, there can be little doubt, must suffer decomposition from the action of galvanism upon it, its oxigen will be evolved at the positive pole, and its hidrogen ought to appear at the negative side. No such evolution of hidrogen however is to be observed in the common mode of making the experiments; and Mr. Davy, by whom it has been executed with so much care, and on so large a scale, has remarked, that at the negative surface there is no liberation of elastic fluid, nothing being produced there but the metallic globules. It appears therefore to follow almost as a necessary conclusion, if the existence of water in potash be established, that, since hidrogen does not appear in its insulated form, it must enter into the composition of the metallic matter, which appears at the negative side; and of course a different view must be given of the operation from that which regards it as a mere decomposition of the alkali.

Simplest mode of investigating this point.

There are several modes of investigation, by which the inquiry with regard to this may be prosecuted. The simplest experiment however, by which light may be thrown

on

on it; and which, as it appeared not difficult to execute with accuracy, I preferred; is to ascertain the nature of the product of the oxigenation of potassium. It was ascertained by Mr. Davy, that, when potassium combines with oxigen, it forms solid, and apparently unusually dry potash. Now if potassium is simply the base of the alkali, forming it when united with oxigen, the potash formed in this way must be the pure alkali: there is no source whence any water can be communicated to it, and of course it cannot contain that portion of water, which is essential to the potash prepared by the common process. But if it be found to contain water, it may be inferred, that hidrogen had existed in the composition of potassium, which, combining with oxigen during the combustion, formed this water.

Mr. Davy's calculation of the dryness of potash formed from potassium,

Mr. Davy had perceived the importance of the inquiry with regard to the presence of water in potash, and endeavoured to determine the relation of the dryness of the potash formed from potassium to that existing in some of its saline combinations supposed to be free from water. Berthollet had inferred, that potash which had been in fusion parts with 13·89 of water when it enters into combination with muriatic acid. By converting potassium into muriate of potash in muriatic acid gas, Mr. Davy obtained results, whence he inferred, that the alkali, which Berthollet had taken as a standard, contains 9 per cent more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, so that the fused potash of Berthollet contains consequently near 23 per cent of water; and he adds, that, from his own observations, potash kept for some time in a red heat contains 16 or 17 per cent of water, taking the potash formed by the combustion of potassium as the dry standard.

liable to uncertainties.

These conclusions however become uncertain in consequence of the different theory, which Mr. Davy has proposed on the action of muriatic acid gas on potassium: they are liable too to the uncertainties, which arise from the difficulty of estimating the quantity of water in muriatic acid, and the subject must be investigated under another form.

Comparison of the neutralizing powers

The following appeared to me to be the simplest mode of experiment, that in which the conclusion is most direct,

of fused potash and potash from potassium.

and the investigation least liable to errour—to determine if there is the same quantity of real alkali in the potash formed by the oxigenation of potassium, and in the potash prepared by the usual process, and which has been kept in fusion, by comparing their powers in neutralizing an acid.

The difference very trivial.

I dissolved 10 grains of pure potash prepared by the usual process, which had been kept in fusion in a low red heat, in half an ounce of water, and neutralized this by the addition of nitric acid previously diluted with 20 parts of water: 165 grains of this diluted acid were required for neutralization. The dry product of the combustion of potassium was then dissolved in water, there remained undissolved a minute portion of matter—On removing this and weighing it, the remaining solution required for neutralization of the above diluted acid a quantity which was equivalent, t a mean of several experiments, to 169·5 grains to 10 grains of the product, supposing it to have been free from any any intermixture of insoluble matter. Here then the same quantity of acid was required for neutralization as nearly as can be expected, for the difference of 4·5 grains of an acid so much diluted on so large a quantity as 169 grains is far from being equal to the difference, which ought to have been found, were the product of the combination of potassium the real alkali, free from water; and is indeed so trivial, that it may nearly be neglected.

Water to be expected in the potash from potassium.

That this would be the case might nearly be inferred a priori; for in decomposing potash by galvanism, there is no appearance of the water which it contains being dissipated. Either that water therefore in its entire state, or its base, must exist in the product; its oxigen, according to the latter supposition, being given out; and of course when that product is again converted by oxigenation into potash, the water must be found in this potash.

Desirable to heat it with iron filings.

An interesting experiment in confirmation of the above results would be to submit the dry product of the oxigenation of potassium to the experiment made by Berthollet on common potash—heating it with iron filings to ascertain if it affords hidrogen. This I intend to do, when I have procured a sufficient quantity of potassium to admit of the experiment being made with accuracy.

From

From the present investigation it appears to be established, that potassium contains hidrogen. This rests on two facts; *first*, that potash, which is proved to contain water, gives out no hidrogen, when converted into potassium by galvanism; and *secondly*, that potassium in combining with oxigen forms potash containing water.

Potassium apparently contains hidrogen.

Gay-Lussac and Thenard, soon after the publication of Mr. Davy's discovery of the metallization of the alkalis, proposed a different theory of the process from that which he had advanced. They considered the new metals as compounds of the alkalis with hidrogen. Mr. Dalton has embraced this view, partly from the consideration of the presence of water in fused potash, and partly from the consideration of the levity and volatility of potassium. The facts above established, it may be supposed, are in conformity to this theory. They may no doubt be explained by it, while they seem not reconcilable to the hypothesis of Mr. Davy. I would propose however a different view, equally adapted to these phenomena, and as it appears to me more conformable to analogy.

Hypotheses of Gay-Lussac, Thenard, and Dalton.

I would suppose with Mr. Davy, that, in the production of potassium and sodium, the potash and soda are decomposed; the oxigen, which I also suppose combined with them in these alkalis, being disengaged at the positive side. But instead of regarding them as the real bases of the alkalis, I would consider them as compounds of these bases with hidrogen. While the alkali is decomposed by the galvanic action, the water it contains must equally be decomposed; the oxigen of both will, in conformity to the law which regulates galvanic decomposition, be attracted to the positive side, and at the negative side the metallic base of the alkali will be evolved; and the hidrogen of the water, being there also in conformity to the same law disengaged, will in its nascent state combine with that base. The potassium or the sodium is the product of this combination. The theory of the production of these substances in the decomposition of the alkalis by iron at a high temperature, in conformity to these views, must be obvious—the iron attracting the oxigen both of the alkali, and of the water, and the hidrogen of the water combining with the real metallic

Mr. Murray's.

The metalloids formed by the interme ium of iron probably contain a smaller propor-

tion of hidrogen.

metallic base, the potassium obtained by this process has been observed to have a greater specific gravity, than that obtained by the action of galvanism. This has been ascribed to a slight alloy of iron, which has not however been detected. It is not improbable, that it may be owing to its containing less hidrogen, a quantity of hidrogen being disengaged in the process by which it is formed, while no evolution of this principle is observed in the production of potassium by galvanism. The whole hidrogen of the water therefore, which existed in the potash, will not be contained in the potassium formed in this way; and to this may be owing that slight superiority of alkaline strength, observed in the above experiments, of the potash formed from potassium.

One or other of these theories—either that proposed by Gay-Lussac and Thenard, or that which I have stated, it appears to me must be adopted; and the latter farther appears to me in several respects preferable to the former.

Arguments in favour of the author's hypothesis.

There is no analogy in favour of the assumption, that the alkaline metals are compounds of the alkalis with hidrogen, but rather an improbability. There are no grounds whence we can infer, that the common metals are compounds of what we at present regard as their oxides with hidrogen; and unless we make this assumption, there is an improbability in the hypothesis, that the alkaline metals, which have all the metallic properties, are compounds of this kind.

An analogy can be traced connecting the earths with the common metallic oxides, and a similar analogy connects the alkalis with the earths. This leads to the conclusion, that all these substances are of similar chemical constitution; that the earths and the alkalis therefore, like the metallic oxides, are compounds of metallic bases with oxigen.

The hypothesis I have advanced is in conformity with both these analogies; it has therefore all the advantages, which belonged to the theory given by Mr. Davy; and, independent of the above facts, which appear to be subversive of that theory, it has some advantages which Mr. Davy's cannot claim. In particular the levity of the alkaline

line metalloids, which has always been regarded as an anomaly, may be supposed to arise from the presence of hidrogen.

That these substances should retain metallic properties, supposing them to be metals combined with hidrogen, cannot be regarded as an objection. Inflammables in uniting with metals do not altogether subvert, and frequently even do not materially modify the metallic properties. Steel, the compound of iron and carbon, has all the qualities of a metal. Sulphuret of iron has the lustre, opacity, and hardness of a metal; and sulphuret of lead or galena has nearly all the external properties of lead, and is heavier than several of the metals. Phosphuret of copper has a high degree of metallic lustre and hardness, and is susceptible of a fine polish. With these facts, there is no difficulty in the supposition, that, from the combination of hidrogen with metals, the general metallic properties may not undergo much change. Besides, hidrogen may be itself a metal, and these may be alloys.

No reason why hidrogen should destroy metallic properties,

if not itself a metal.

If these conclusions be just, it follows, that the metals which are the bases of the alkalis are still unknown to us in their pure state. The same conclusion follows perhaps with regard to the metallic bases of the earths. Nor is it impossible, that some of the common metals may contain hidrogen. These conclusions, it must only be remarked, rest on grounds altogether different from the hypothesis which has been proposed, that hidrogen is the common metallic principle, and have no connection with it. It is a conjecture supported at present by no species of proof. I do not suppose, that hidrogen is essential to the constitution of metals; but merely, that in common with other inflammables it is capable of combining with them; and according to this opinion there must exist in the alkaline metals, metallic bases independent of hidrogen.

Metallic bases of the alkalis yet unknown to us.

No proofs, that hidrogen is the general metallic principle.

The peculiarity which distinguishes the alkalis, regarding them as metallic oxides, and which undoubtedly presents an apparent anomaly—their great solubility in water, may be owing perhaps to the attraction which their bases have to hidrogen; for the affinities of a substance to a compound, to water for example, it is probable according to Berthollet's

Solubility in water of the alkalis, considered as oxides.

views

views of affinity, are the modified affinities of that substance to the elements of the compound*. If the alkaline metals be admitted to exert an attraction both to hidrogen and oxigen, but more energetic, as their chemical relations evidently show, to the latter, it will follow, that the more powerful affinity will first be exerted, or the metal will combine with oxigen. But when this is diminished in force by the approach to saturation, the weaker affinity, that to hidrogen, will be capable of being exerted with equal effect—the attraction therefore of the oxidated base or alkali will now be exerted both to oxigen and hidrogen, in other words to water, with which it will combine. If the bases of barytes, strontites, and lime, be procured pure, they may be expected also to contain a portion of hidrogen, though perhaps to a less extent than the alkaline metals, as in their state of earths they are less soluble in water than the alkalis—indicating, according to this view, a less strong attraction exerted to hidrogen. There may thus be a series, through the other earths, to the common metallic oxides.

Source of the properties that distinguish the alkalis, earths, and metallic oxides.

And if this speculation can be admitted, we have the satisfaction of being able to point out the source of the peculiar properties, by which the alkalis, earths, and metallic oxides are distinguished as subordinate divisions of a class; and of farther showing that the cause giving rise to these peculiarities is the same as that which is the cause of the equally striking differences by which they are distinguished in their simple or metallic form. The attraction of the real alkaline metals to hidrogen gives rise to their combination with it, and this is the cause of the levity, and probably also of the fusibility of potassium and sodium—the most striking properties by which they are distinguished from the common metals; and the same affinity to hidrogen is the cause of the solubility of these substances, when oxidated, in water, the property by which the alkalis and alkaline earths are separated as an order from the common metallic oxides. If this view be correct, it will also follow, that the metalloids obtained from the earths containing only a small por-

* I have given at considerable length a view of this doctrine, which affords so many refined explanations of minute chemical phenomena, in the Notes to my System of Chemistry, vol. I, p. 609.

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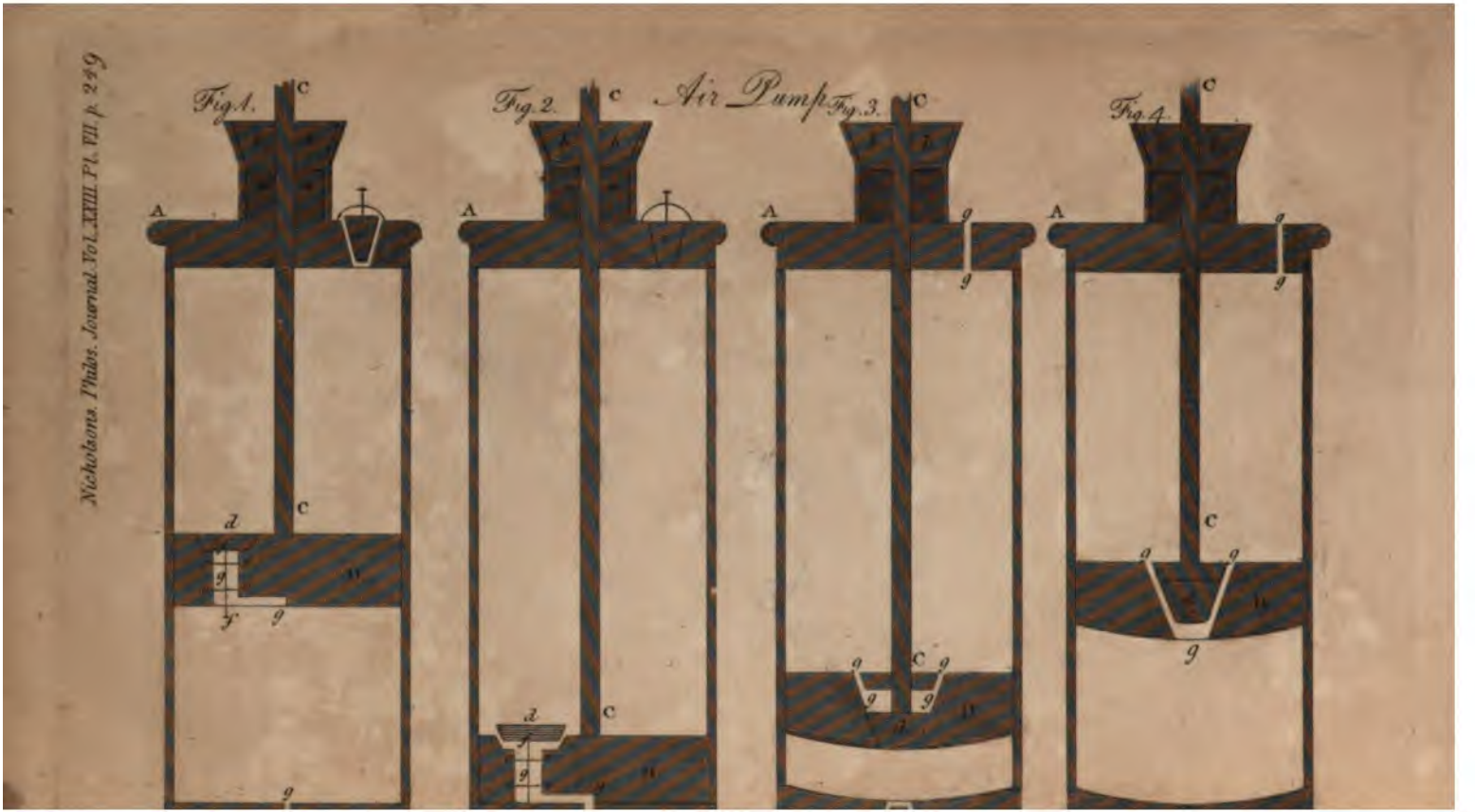
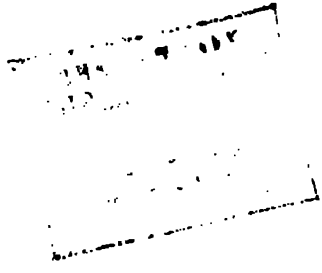

Nicholsons. Philos. Journal. Vol. XXIII. Pl. VII. p. 249
Fig. 1.
Fig. 2.
Air Pump
Fig. 3.
Fig. 4.
A
C
d
g
f
B



tion of hidrogen will be of intermediate specific gravity between the alkaline metalloids and the common metals, and that the earths themselves must have a degree of solubility in water, intermediate between the alkalis and the common metallic oxides.

I am, with much respect,
Yours &c.

Edinburgh, 21*st Feb.* 1811. J. MURRAY.

II.

Cheap and simple Construction of an Air Pump. In a letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

AS you were so good as to publish a paper, which I sent to you some months ago, on a method of joining long or bent metallic tubes, &c., I have taken the liberty of troubling you with the following account of some attempts, which I have made, to simplify the construction of the air pump. Not being willing to go to the expense of one of the best air pumps, and at the same time equally unwilling to spoil my experiments by a bad one; no other alternative remained, than either to improve the old construction, or to invent a new one. How far this attempt may have succeeded, I must leave to the judgment of yourself, and of your readers. Attempts to construct a cheap air-pump.

The following plan was the first that struck me: Let A B. (Pl. VII, fig. 1 and 2), represent the barrel of the pump; C C the piston rod, which is to be made solid, either of brass or steel, and accurately ground; and D the piston, which is made to move air tight in the barrel, by means of leathers as usual. The rod also moves in a collar of leathers, *a a*, and *b b*, is a cup to hold oil—so far the construction is the same as in Smeaton's air pump. The alteration which I have made is in the valves, which are of solid brass; 1st. construction.

c is a conical brass valve, well ground into the top of the barrel, on one side of the collar of leathers, and kept from being thrown out of its place, when open, by a pin and a small semicircle of brass. *d* is another brass valve, only this must be made much thinner than the other, that it may open with more ease by the condensation of the air below it. This valve has a long wire, *f f*, fixed to its under side; which wire, when the valve is shut, must project about $\frac{1}{4}$ of an inch below the piston. *e e*, *e e*, are two cross pieces, having a hole drilled through the middle of each, through which the wire *f f* passes. (One of these is represented separate at fig. 2, Pl. VIII.) When the valve *d*, is open, it is prevented from flying out of its place by a small button, at the end of the wire *f f*. *g g*, *g g*, are the passages by which the barrel communicates with the receiver. After the valves are finished, and fixed in their places, the top of the piston, and the under-side of the top of the barrel, are to be ground accurately to each other; so that when the piston is drawn to the top of the barrel, there may not be the least vacuncy between them. The action of this pump will be as follows:

Action of this pump. When the piston is moving upwards, the valve *d* will shut by its own weight, and by the condensation of the air above it, which having no other exit, will necessarily open the valve *c* in the top of the barrel; and when all the air is expelled, this last valve will also shut by its own weight; and as the top of the barrel and the piston are ground to fit each other, it is evident, that the *whole of the air*, above the latter, must be expelled. When the piston descends, the valve *c* (fig. 2.) will still continue shut, by its own weight, and *now* also by the pressure of the atmosphere, and the valve *d* will open, by the condensation of the air below it, and in the receiver; air from the latter, therefore, will rush through the passages *g g*, *g g*, into the upper part of the barrel, and will be expelled again when the piston is again drawn upwards; and this expulsion of a fresh barrel full of air, every time the piston ascends, will continue, till the air in the receiver is so far rarified, as not to be able to lift the metallic valve *d*. It is now that the use of the wire *f f* will be manifest, for by projecting below the piston, it is evident, that it must reach the bottom of the barrel, sooner than the

piston

piston does; and as the piston continues to descend, the valve *d* will be opened, by the pressure of the wire *f f* against the bottom of the barrel, and the air in the receiver will expand freely into the barrel, through the passages *g g*, *g g*, as represented in fig. 2. This air will also be expelled by the ascent of the piston; and this operation may be repeated as often as is thought necessary. As the valves will not be exposed to much friction, they will probably keep in order a long time; and if the pump is well made, there seems to be no impediment to its producing a perfect exhaustion*. But whatever care the workmen may take, it is probable, that a minute quantity of air will remain, and that in a state of considerable condensation (under the collar of leathers) between the piston-rod, and the top of the barrel; and also between the sides of the barrel, and the upper part of the piston: but this is a fault, to which every pump, I believe, is liable. It may also be objected, that, when the valve *d* is opened, by the pressure of the wire *f f*, against the bottom of the barrel, the air, by rushing suddenly out of the receiver, will occasion oscillations of the mercury in the gage: these oscillations, however, must be very trifling, not exceeding $\frac{1}{4}$ of an inch, as the valve *d* would open, were the pressure greater than that of $\frac{1}{4}$ of an inch of mercury. All these imperfections however, I hope, are completely obviated, by the following construction.—

2d construction.

At figs. 3 and 4, A B, as before, represent the barrel, C C the piston-rod, D the piston; *a a* the collar of leathers, *b b* the cup to hold oil, *c* the expelling valve of brass, *d* the valve in the piston of brass also, and *g g*, *g g* the passages

* It is obviously impossible, that the whole of the air in the receiver should be taken out in this way, so as to form a *perfect* exhaustion, provided we admit the workmanship carried to the highest degree of accuracy, so as not to have the slightest imperfection. Suppose the barrel and receiver to be of equal capacities. The first time the piston is drawn up, the air in the receiver will expand to double its former volume, or be of half the density it was before. After the second stroke of the piston, it will in the same manner be of half the density it was after the first. And thus the rarefaction will go on in a series decreasing in geometrical progression, which can never reach 0, as the last term must be half the value of that preceding. C.

by

by which the barrel communicates with the receiver. This pump differs from the former in the following particulars: first, in communicating with the receiver through the *top* of the barrel: secondly, in having the valve, in the piston, fastened to the end of the piston-rod: and thirdly, in the bottom of the piston being convex, and the bottom of the barrel concave. After the valve *c* and *d* are finished, and fixed in their places; the convex and concave surfaces of the piston and of the barrel are to be well fitted by grinding; so that, when they meet, all air may be excluded from between them. The reason for making the bottom of the piston convex, is, that the air may be prevented from lodging between the piston, and the sides of the barrel; as the leathers, by this construction, will reach the bottom of the barrel; for the bottom of the piston being convex, it will have no assignable thickness where it comes into contact with the sides of the barrel; as is shown at *k k*, Pl. VIII, fig. 3, the dark part representing the leathers. This pump will work as follows.—

Its action.

When the piston-rod (fig. 3) is pushed down, the valve *d* will shut by the pressure; and the air below it, having no other exit, will be forced out at the valve *c*, which will shut, by the reaction of the brass spring *l l*, as soon as all the air is expelled; and when the piston is again drawn upwards, it will continue shut, for the same reason, and also by the pressure of the atmosphere.—As the valve *d* is fixed to the end of the piston-rod, it will be impossible to draw up the piston, without first opening this valve; and by this means establishing a communication with the receiver, as represented fig. 4, by the same means also, the oscillations of the mercury in the gage will be prevented: during the ascent of the piston, therefore, the air will pass freely, through the passages *g g*, *g g*, into the lower part of the barrel; and when the piston is again pressed downwards, the first effect of the force applied will be to shut the valve *d*, and the air below it will be expelled at C as before.

I have had a pump made of this latter construction, and find it to answer my expectations, in every respect, as far as the principle is concerned; but I have never been able, entirely to prevent the different junctures of the pump (which

(which were secured with oiled leathers, in the usual manner,) from leaking: but, even with this imperfection, the mercury has frequently descended, in the siphon-gage, to within $\frac{1}{10}$, and once or twice to $\frac{1}{20}$ of an inch of the level. I have thought of a plan of obviating this imperfection, (to which, by the by, every pump is more or less liable) and shall be happy in communicating it to you, if it answers my expectations. There is one objection, which may be made to this pump; and that is, that the valve *d* will be exposed to considerable friction. This may be obviated, in some degree, however, by making it of iron, or steel, which is known to wear very little in brass; and still farther, by fixing it loosely to the end of the rod, in the same manner as the valves are fixed to the keys in Potter's patent flutes; or it may be entirely removed, at the expense of a trifling inaccuracy in the pump: as follows. C, (Pl. VIII, fig. 3,) is the piston-rod, D the piston, and *m* a brass plate, fixed to the end of the rod, instead of the valve *d*: to this plate a piece of leather is screwed, which is shaded darker in the figure; this leather, when the rod is pushed down, covers the hole *g g*, and prevents the return of the air into the upper part of the barrel. The hole may be made so small, as not to contain, in a barrel of 10 inches capacity, more than ·00001 of the air, independent of the size of the receiver. I have had a pump constructed with a valve of this description; and find it to answer perfectly well. It may be farther improved, by fixing a small wire, or valve, to the brass piece, (as is shown by the dotted lines) which will fill the hole *g g*, when the piston is pressed downwards: only care must be taken, that it do not project so far, as to open the valve *c* in the bottom of the barrel. Its action

The price of a single-barrelled pump, of this description, (as represented, Pl. VIII, fig. 1,) made by Messrs. Dollond, will not exceed 10 guineas. It may also be made with two barrels. It is evident, that this pump will also act as a condenser, with a very little additional apparatus; which, as it will not differ from that usually employed, it is unnecessary to describe. I must beg leave to add, that the juncture, I formerly recommended, for bent tubes*, will be

* See vol. XXV, p. 284.

very

the next and last step is the tearing off the limb, exactly as nature does it. Perhaps no appearance shows the formation of trees and shrubs, and explains their construction so well, as this way of examining them; it often points out the line of life drawn after the bud; shows many a bud creeping through its undulating prison; and above all exemplifies that curious construction, which I shall soon explain by the name of the minor obstruction. These studies, with a vegetable cutting taken from two or three different parts of the plant under investigation, formed the specimens from which I proposed judging of the interior of plants. I have already drawn 86 trees, and 123 shrubs, in this manner: but it is only the renewal of former labours, though the last four months have more particularly been dedicated to it. If I wished to recommend to my sex a complete cure for the vapours, I could not offer them a better than that, which, having employed me 13 hours out of the 24, never gave me a moment's weariness. How, indeed, is it possible to be tired of contemplating such wonders, and discovering such miracles! Enthusiasm here is the natural consequence of seeing.

The line of life.

The first thing necessary to the explanation of the dissection of the exterior of the stem of trees, shrubs, and smaller plants, is, that I should ascertain the existence of that important part, the line of life; which has by some means missed the observation of most physiologists. Du Hamel indeed (that excellent writer) observed, that all buds proceeded from it, and recommends it to the attention of future botanists. Linnæus (whose every hint is a trait of genius and science) finding that all buds, and all radicles, proceeded from this part, and not seeing the line of life, supposed it was from the pith they came; for the line of life is only to be distinguished by a pretty strong magnifier; but it is very easy to be discovered in all common vegetable cuttings, and still more easily traced from that line, which all philosophers have agreed to own of the first consequence in the plant, and which they call the impregnating vessels; but which is the identical line of life, which I have repeatedly taken out without breaking from the stem to the seed, and from the embryo back again to the stem. Its identity

will

will not therefore admit of a doubt; though I must say it would have appeared to me a greater miracle, that so important a part should do its office and then disappear, than that it should continue its consequence throughout the whole life of the plant. Besides, what would become of the buds formed in the trees? It is a fact easily ascertained, that no part can give life without being visited by this line; neither leaf nor leaf-bud, no part of the root, no part of the stem, but where this identical vessel is found: it runs up each bud to prepare it, as it does in the seeds, and the bud becomes capable of giving life; because it communicates the same string. Is it not proved, when a leaf-bud by an ignorant gardener has been mistaken for a flower-bud in budding? Du Hamel in vain endeavoured to make them grow; he bestowed the greatest care, but without effect; it was impossible, for they would not cement, not having that line, which first joins them together. Not so the radicle; possessing that line, which runs through it, if well managed, it will grow with ease. Those large buds also, which the roots of various plants throw up in the spring, such as the peony, &c., if the root did not send up the line of life, how would they continue their progress? like an excrescence they would decay and moulder away without bringing forth buds. To fix the absolute certainty of this, is of the utmost consequence to botany, particularly to physiology; I shall therefore collect all the reasons, which acted on my own belief, and persuaded me of this truth: viz. that the lines which run between the pith and the wood are really the lines of life, without which the vegetable cannot exist.

It is essential to the vitality of every part of a plant.

1st. It is the line from which all buds whatever take their rise. 2d. It is the line from which all radicles shoot. 3d. It is that line, which in grafting and budding is first seen to join. If the graft misses, this line turns black, and then no power can save the plant. 4th. If in budding it has not power or strength sufficient to join the new bud to the tree, it dies, and the wood makes no more efforts to unite. 5th. When in a frosty morning the apricots and peaches are hurt, and you wish to know whether they will recover; open a few flowers, and if the line of life is black, though the rest of the pistil may be perfectly free from the taint, despair of the

Proofs of its existence.

greatest part of your fruit: it never recovers after this, though the female may be some time before it decays. 6th. The flower bud will inoculate, but not the leaf bud. 7th. The line of life is never found in any leaf, but those which flower in the leaf; but here it is easily detected, meandering as in a flower stem. 8th. Take this line out of the bud when going to inoculate; it is not a quarter of the bud, but the bud grows not after such a dilapidation.—(It is the same line I took out of the bean.)—Every other part of the bud will renew itself, though badly; but this deprivation is sure to kill directly, while in so young a state. It is this line in potatoes, that enables the gardener to cut them into innumerable pieces, provided an eye is there, they will grow: What is an eye? a little knot of this string, which may be seen running from part to part. It is the same in all parting roots: in vain you would endeavour to make any other part grow, as I have often tried, but the smallest knot has the effect. So important is this line to every plant, that it is this which regulates the form of its colour, it is this, which, confining the pith, gives shape to the silver grain, or yearly circles; if this (as in the nerium oleander, or zelanicum,) runs into an irregular form, so also does the yearly shoot; but if in a regular manner it shapes its course, as in the dogwood, or still more exactly in the ephedra disticha, then that part of the plant follows its example with the most scrupulous nicety. The line of life generally consists of two or three rows of circular vessels, often very differently coloured from the rest of the plant, and generally yellow or a faint green in the spring. It has a very peculiar juice, which is thick and sweet, but often very bitter also; and quite different from the juices of any other part of the tree; and it becomes sweeter as it advances to the flower-branch, till it is found almost honey. In the cryptogamiæ and in grasses it is extremely conspicuous, as having (especially in the former) fewer lines to confuse the learner. What more can be said to prove its importance, except what relates to the bud? and to that I shall leave it; and turn to my next subject, mentioning that I include not in the present either water-plants, bulbous-roots, cryptogamiæ, or grasses.

Obstructions

There are three different sorts of obstructions in all plants. The

The 1st I shall call the grand obstruction, to distinguish it. It takes place only in two parts of the plant; that which separates the root from the stem; and that which divides the stem from the peduncle or flower stalk. In both cases the whole order and arrangement of the stem is so altered, that a pupil, accustomed to find six parts in regular gradation in its form, finds only three in the root of all the trees and shrubs. In the stem there are the rind, the bark, the inner bark, the wood, the line of life, and the pith: but in the root only, the rind, the wood, and the line of life. In some herbaceous plants, and annuals, a row of bark is found and some pith: this difference of the root does not take place immediately, but by gradual increase of rows, within the compass of an inch above ground; for they are the parts which have formed the seminal leaves, and the outward case of the embryo, that now grow up increasing in the bark and inner bark, till they arrive at the proper form of the stem; while the wood of the root decreases in like proportion: for the embryo growing up in no wise alters the situation of its parts. That outward case, which formed the holders to the seed, and the cotyledons, again, when out of the seed, forms the bark and inner bark of the stem, returning to the part from which it came; so that nothing can equal the beautiful simplicity of nature's designs, if we did not ourselves load them with difficulties. The root is certainly a reservoir for nourishment, which the radicles bring in quantities, which is contained in the wood, and which it dispenses to the tree by the means of the stem. The radicle is formed of the same parts as the root, having the line of life in the middle, ready to dispense to any minor radicle.

in plants. 1st grand obstruction, separating the root from the stem.

The next grand obstruction is that of the peduncle; dividing the flower stem from the branch. Linnæus alone hinted at this; and so plain is it to be seen, that to look is to be convinced. Cut a slice from both, and compare them; and their arrangements will be found wholly different. On examining the wood, it will appear to divide into various little parcels, answering to the number of stamens the flower contains; while the inner bark separates into the number of petals; and the line of life, fixing in the middle, generally denotes the figure and divisions of the pericarp.

2d. grand obstruction dividing the flower stem from the branch.

I have seldom seen this vary; nay, even in compound flowers of the syngenesian order, where the divisions are so often repealed, and the compartments of each perfect flower so very minute, the pin-holes of the stamen surround the larger apertures of the pistil, and, accompanied by the vessels appropriated to each petal, are as exact and as exactly delineated as below the largest flower, a campanula, or a lilly. Let any person see a vegetable cutting of the peduncle of a syngenesian superflua in a solar microscope, and he will have no doubts. This is made still plainer by the bursting of the vessels of a double flower, and the confusion that ensues, one part growing for another, one on another; sometimes the wood escapes bursting, and then the males are perfect; sometimes the line of life does its office till it passes a second time into the pericarp, in the way to the seeds, and then fails; and in this case all is perfect but the seeds, which do not vegetate. In my opinion, it would be a much greater wonder to suppose such different parts to be formed of the same matter, than to conceive that each had its appropriate juices. But I mean not this as an argument. I never allow myself to reason in this manner. I rely on experiment, observation, and dissection only, and I may say, without vanity, on a labour, few would submit to; and none can appreciate but those who know me.

Middle obstruction. Stoppage of the pith.

Having described the grand obstruction, I shall now show the middle one. This is formed by the stoppage of the pith. The pith is most properly esteemed a source of moisture to the tree; but when I come to show the manner of passing of the buds, I shall prove a still more direct use for its moisture. The pith stops in all trees at every new branch, forming a piece of wood, which generally fills up nearly half an inch, apparently to give the plant, by a great accession, strength sufficient to invigorate and produce healthy branches. The pith therefore recoils, not only in the new bud, but in the main stem also. But in smaller plants the new shoot alone generates new wood, which appears an accession sufficient for such slender twigs; where the pith occupies so large a portion of the plant: and in some annuals and herbaceous plants the pith stops not at all, but is contented with drawing the line of the corners of the wood closer;

closer, sometimes adding a small piece to the interior, for the purpose of collecting nourishment. There is, however, a sort of hollow plant, which forms a complete stoppage; but it generally grows in wet ground; and will be noticed with water plants, in another letter. These various differences very exactly regulate many important points belonging to the construction of the interior of the stem, viz. the quantity of new formed wood; the situation, manner of passing, and regulation of the buds; and particularly the formation of the minor obstruction, which I shall now explain.

Minor obstruction. Imperfect stoppage of the pith.

This last seems more particularly to belong to those plants, which, without stopping the pith, a little impede its progress; and the curious construction of which is such, that, had I not already shown it in the French bean, I should have deferred mentioning it here, till better able to comprehend its structure. I have found the marks which indicate the aperture in the largest trees. In the Weymouth pine, and in the silver fir, it seldom fails to give proofs of its excentricity, even at the exterior of the tree; but I never saw the ball, except in those which are running plants, or at least shaped like them; that is, that have the large shooting branches made like the shoulder of a man, with a ball and socket*. I at first conceived, that in these small plants the branches were to turn on the ball; but I have found many specimens, where the limb was too firmly fix'd for this purpose. I then conceived it the foundation for tendrils; but I have found it where no tendrils are: besides, it is too diminutive. Not able to dedicate more than one figure to this service, I must delay many extraordinary proofs of its existence; and many curious peculiarities of its structure, on account of the buds, which demand all the sketches I can venture to give. I shall only say therefore, that it is fixed in the under part of the branch, where no bud ever comes, but is defended with the greatest care.—(See Pl. IX, fig. 1.)†

* I understand that I have been thought to commit myself in comparing a part of the seed to the breast of a bitch, having disapproved of animal comparisons; but it was a long analogy I found fault with, not a simple likeness in form, and surely there is a great difference between such a one, and an analogy reaching to perspiration, circulation, and even perception

† This, with some other figures belonging to the present paper, is ob-

In

In my next, the reader shall know as much as I am acquainted with, and that is but little.

Annual formation of wood.

In all trees and shrubs, one row of new wood is formed each year; but this is not all, some new wood is always generated to accompany each bud. Small plants, however, the pith of which is little, if at all separated; and where the line of life runs from the middle of the pith in various vessels to the exterior, all form two, if not three rows of wood each year. Cabbage, and its offspring, also turnips, carrots, &c., show one for every time of flowering, whether dilapidation takes place in the part above, as in the cabbage and cauliflower, &c., or not. The same accession of new wood takes place in the root also. There are many very extraordinary points of construction in these small plants; such as the bastard vessels running often through the pith, and across to the wood at the other side; and innumerable other things, which, belonging rather to peculiar plants, than to nature in general, I shall therefore leave to the next letter, and turn to my last and chief subject.

Buds pass through the wood.

On this subject, it is quite astonishing to me that any one should differ, since the evidence is so exact and plain. If we look at the floor of a room, a piece of wood, a tree, &c., they should all together or separately teach us, that the greatest part of the buds must pass through the wood; since there is no wood that is not marked with them to the edge of the pith, that is to the line of life. In one of my former letters, I gave a long account of the formation of the leaf-bud, mentioning how much it differed from the flower, or leaf and

Formation of the leaf-bud,

flower-buds. Formed almost wholly of the bark and inner bark, and placed there, it is the lengthening of the threads of the bark, which weave the leaves; and all the time they are forming, the bud remains stationary; nor do they draw any assistance from within, except a little wood in the shape of nourishing vessels, which surrounds the spiral wire. But

and of the flower-bud.

very different is the formation of the flower-bud. Botanists have perceived this difference, and yet appear to derive them both from the same source. Most physiologists have considered the bud as proceeding from that row of new wood,

bliged to be deferred to the supplement, which will be published the 1st of next month.

which

which is formed each year next the bark. If so, how comes the bud into the farther part of the wood? The destination of the bud is to spring without; and to this part it will of course proceed by the shortest road, so that, if it began at the albumen, it would soon attain its situation. But the flower bud, and leaf and flower bud, are really formed on the line of life. When a bud is going to shoot, the line of life forms a knot, and a quantity of albumen is directly generated round it; and the knot covered by a scale. While this is doing, the wood is preparing for its reception. Some parcels of the old wood, sinking before it, form a bed; and others, rising above it, prepare a sort of arch over it, in the middle of which it passes, as in a vaulted cave without hindrance, or pressure. I have followed hundreds with a sharp knife; and if this is done without injuring the under part, it will be perceived to shine below where the bud is to go, as if varnished. Probably the line of life brings with it a softening liquid for this purpose; and this line is placed next the pith, that the latter might afford the bud plenty of moisture to support and invigorate it in its first shooting, and during its journey to the exterior of the tree. Besides, I should suppose that moisture would much contribute to bending the vessels of the wood into the shape required; basket-makers moisten continually their twigs, to bring them to the necessary form; and nature in this respect acts as the basket-maker. The best sort of wood for seeing the progress of the bud is a pollard, cut for the second time, and then taken off about February. The buds then are springing from the line of life in numbers. I have a beautiful specimen of a willow, where, from the quantity of buds egressing, the wood forms a complete scollop the whole way. (See Pl. IX, fig. 2.) It is probable, that these may be the buds of the summer; though I have many reasons to believe that a bud is not near so long growing as is generally supposed. When the bud has arrived at its destination in the bark, it stops; the leaf and flower bud to form its leaves, which it does before the flowers; and both to gain those scales and clothing, that enable them to bear the outward air; and, if the air is not mild enough, they remain till it is fit. Every accession of wood at the exterior, forms a sort

Best seen in a pollard.

sort of cradle for buds, where often two or three are found resting, and completing their scales, and fiting themselves for their entrance into the world. It is this situation, that has been taken for their birth; but it is only their place of repose after their long journey. In young wood there is so much pith, that they have but little way to travel; but in old wood it is very curious to behold the length of way through which they will go, the wood-vessels arranging themselves in the most exact order, to let them pass. In the yew, where there are so many buds, they will continually derange even the yearly or silver grain, and oblige this also to prepare them a proper and hollow passage. In all plants where the pith stops not, the line of life runs in the middle of the pith, constantly making its way to the exterior, with the buds. I have in some annuals, and some herbaceous plants, good specimens, where the buds are passing in different directions. I shall give one of the malvas (See fig. 5).

Hard mortar penetrated by a vine-bud, and stone by a fig-tree.

At first it appears impossible, that so soft a substance as a bud should make its way through such a quantity of hard matter; but many instances have been known, where vine-buds have passed through the hardest mortar; and I once saw the branch of a fig-tree, which had made its way through Bath stone. But in the present case it is not the bud, which forces forward: it is the wood, which in a far more extraordinary way prepares for its reception, by the preceding of even a softer substance than itself, albumen. Without absolutely saying so, we are very apt to set bounds to the power of God; and circumscribe it within our capabilities; yet every day's study should convince us, that it is only little by little, that we can even comprehend the glorious works of the Creator; and the astonishing power he has shown in this beauteous world he has formed.

The bud will not permit any thing in the nature of wood to turn it from its course, which is always the shortest line that can be drawn from almost the centre to the circumference. I once saw a horse-shoe, which had by some means got into the wood; the buds in vain essayed to pierce it, they all died in the act of trying; some of them made an effort (being stronger) to pass round it; but died also before they could reach their destination: at last the line of life

Air Pump.

Fig. 2.

Fig. 3.

Structure of Plants.

Fig. 4.

Fig. 5.

Fig. 6.

life began to form with a new pith, and the part, where the shoe was, decayed; but in this state the tree was cut down, or it would have produced a still more instructive history of the force of buds; which had for three years contested the point continually with this hard substance.

All that farther concerns this subject is more peculiar than general; I shall therefore defer it to my second letter. I was anxious indeed to clear my way, that, having laid the foundation, I might proceed to the many wonderful parts, which I shall hope to analyze in my next.

I am, sir,

your oblig'd humble servant,

AGNES IBBETSON.

Explanation of the Plates.

Pl. VIII, fig. 4. The bud extremely magnified, the wood passing over and under.

Fig. 5. Section of the malva, showing the bud on the line of life (*k*) the two buds: (*kk*) the albumen in the herbaceous plants, but it is seldom so regular in form.

When the bud is to be formed, the line of life makes a knot; this knot is directly covered with a scale, and both within and without with the albumen, which grows; and I do not think there is any other increase of the bud, till it arrives at its cradle in the bark.

It may be thought, that there are repetitions of my former letters, but to describe the outward part of plants, and afterwards the inward, it is scarcely possible to avoid repetitions, anxious as I am to be perfectly understood as I proceed.

I have given also a specimen of a vegetable cutting of the cactus, (fig. 6.) to show the curious manner in which the buds follow each other, and are discovered on the line of life. *l l* the bud.

Pl. IX, fig. 1. The appearance of the minor obstruction, and the sort of plants that more peculiarly show the ball, exhibited in scrophularia. *a* the ball, *b b* the bud, or, when it is wanting, the cradle where it reposes, *c* the hollow of the pith.

Fig. 2. Appearance of the buds in a willow.

Fig. 3.

Fig. 3. A piece of wood torn off the branch, showing the minor obstruction just below the pith.

Fig. 4. A piece of wood cut lengthwise, with several buds, *d d*, travelling from the line of life to the exterior, through the wood; *e e*, buds travelling through in another direction; *f f*, the new wood, or albumen.

IV.

Observations on the Oxides of Iron, with a Discussion of their Nature. By Mr. J. H. Hassenfratz.

(Concluded from p. 193.*)*

FIFTH METHOD. *Oxidation of Iron by acids.*

Oxidation of iron by acids.

WE know that iron is oxided when dissolved in acids, 1st, from the gas that is evolved; and 2dly, from the nature and state of the precipitates after the solution. We will examine, by each of these two methods separately, what proportion of oxigen combines with the iron, to promote its solution.

Of the quantity of oxigen deduced from that of the gas evolved during the solution.

Calculation of the oxigen from the gas evolved.

According to the nature of the acids the gasses collected will be different. When the iron is dissolved in nitric acid, the acid itself is decomposed, its oxigen unites with the metal, and the nitrous gas, that was combined with it, is evolved. In concentrated sulphuric acid too, the acid is decomposed, and sulphurous acid is disengaged: but in sulphuric acid diluted with water, muriatic acid, acetic acid, &c., it is the water that is decomposed, and hidrogen gas is obtained.

Lavoisier with nitric acid.

Lavoisier dissolved iron in nitric acid, collecting and measuring the nitrous gas evolved*. Having ascertained by previous experiments the quantity of oxigen, with which this gas must be combined to form nitric acid, he thence deduced the quantity combined with the iron during its solution. He found, that the results were very variable; and

* Mém. de l'Ac. des Sc. 1782, p. 492 and following.

that

that he obtained more or less gas, according to the temperature at which the experiment was made. From some experiments he inferred, that the proportion of oxigen combined with 100 parts of iron must be 32. In one experiment, made between 25° and 30° of R. [88° and 100° F.], 32·27 of oxigen combined with 100 of iron *. Notwithstanding these differences, he conceived the quantity of oxigen combined with iron dissolved in nitric acid could be carried only to 29 per cent of iron.

Objections to this method.

It is difficult to estimate by this method the proportion of oxigen in a constant state of the oxide, because, 1st, the proportions of gas obtained vary with the temperature and the degree of concentration of the acid; 2dly, it is extremely difficult to obtain a uniform nitrous gas; 3dly, the iron is always oxided in different degrees.

Bergman with sulphuric and muriatic acids.

Bergman †, having dissolved separately more than forty varieties of iron in sulphuric acid diluted with water, and in muriatic acid, obtained with the former from 36 to 52 Swedish cubic inches of hidrogen from the docimastic centner of iron, and with the latter from 15 to 51 inches.

Priestley.

Guyton quotes an experiment of Priestley, in which the English philosopher obtained 155·5 cubic inches of hidrogen gas with 27·55 [grs.] of iron; which would carry the quantity of oxigen combined as far as 39·7 parts to 100 of iron ‖.

Lavoisier.

Lavoisier, having dissolved 100 grs. of iron in dilute sulphuric acid, obtained 110 cubic inches of hidrogen gas, which would make the quantity of oxigen combined with 100 of iron 27·3.

Vandermonde, Berthollet, and Monge.

Vandermonde, Berthollet, and Monge, dissolved 21 varieties of iron in sulphuric acid diluted with water. From the purest iron they obtained 78 ounce measures of hidrogen gas, at the temperature of 12° R. [59° F.], and pressure

* Ib. p. 497. † Analysis of Iron.

‖ Not being able to turn to the passage in Priestley, as the reference is to the Encyclopédie, I cannot verify the numbers here given. Mr. Hassenfratz appears to have taken the numbers as representing French cubic inches, and French grains; but supposing them to be English inches and grains, which is very probably the case, the proportions will no give more then 27·69 oxigen to 100 iron. C.

28 in. [29·8], which would make the quantity of oxigen combined with 100 of iron 29*.

Vauquelin. Vauquelin, having dissolved several kinds of steel in dilute sulphuric acid, obtained from 108 to 121 cubic inches of hidrogen gas; whence the quantity of oxigen combined with 100 of iron would be from 26·6 to 29·9 †.

Experiments to be excluded. From the products of hidrogen gas obtained by dissolving iron in dilute sulphuric acid we must exclude those of Bergman, which exhibit too great differences to deserve any confidence; and that of Priestley, which appears much too considerable not to induce the suspicion of some errour, or that great part of the iron was oxided at a maximum ‡.

Conclusion from the others. The other experiments lead to the conclusion, that the quantity of oxigen combined with iron varies between 26·6 and 29·9 to 100 of the metal dissolved.

But iron is seldom pure Iron is seldom pure, always containing carbon, scoriæ, and sometimes oxidule. The iron that gave Vauquelin 121 cubic inches of hidrogen contained 0·00633 of carbon, and 0·00626 of siliceous earth.

so that we may estimate the oxigen at the highest. The impurity of iron leads to the presumption, that the quantities of hidrogen obtained were less than would have been produced, if the metal had been perfectly pure; whence we may consider the greatest quantity of hidrogen obtained as resulting from the purest iron: and that analysed by Vauquelin contained at least 0·01 of foreign matter, so that we may carry the proportion of oxigen combined with 100 of iron dissolved in dilute sulphuric acid as far as 30 without danger. But, according to Thenard, the iron in this solution is in the state of white oxide; whence the white oxide must contain 30 of oxigen to 100 of metal, or consist of 77 parts iron and 23 oxigen.

Thenard's white oxide.

Of the quantity of oxigen combined with iron ascertained by the oxides contained in the solutions.

Calculation of the quantity The quantity of oxigen contained in the oxides separated from their solutions may be ascertained in three different

* Mém. de l' Ac. des Sciences, 1786, p. 166 and following.

† Journ. des Mines, No. 25, p. 23.

‡ If my conjecture, expressed in a preceding note, be right, this remark on Priestley's experiment is unnecessary. C.

modes:

modes: 1st, by distilling the solution to dryness, washing the precipitate, and drying; 2ndly, by precipitating the oxide by an alkali; 3dly, by precipitating in the metallic state, by means of iron, the dissolved oxides of other metals, the quantity of oxigen contained in which is already known. in the oxides themselves.

1st *Method.* Lavoisier dissolved iron in nitric acid, evaporated to dryness, and obtained a red oxide, the weight of which was from 40 to 50 per cent more than that of the iron dissolved. Red oxide with nitric acid by Lavoisier;

Bucholz dissolved iron in diluted nitric acid, evaporated it to dryness, and kept it in a red heat for a quarter of an hour. In three successive experiments the increase was 42 per cent. Bucholz,

Gueniveau, mine engineer, writes to me from Paris, on the 30th of September, 1807: "We also have been employed on the same subject (the oxidation of iron by the nitric acid), and we conclude, that the oxide is composed of 100 metal with 44 oxigen. We employed the same method as Mr. Bucholz." This experiment appears to have been repeated in the laboratory of the council of mines at Paris. and Gueniveau.

Iron dissolved by nitric acid is commonly in the state of red oxide. Lavoisier says, that, having calcined this oxide to reduce it to the state of ethiops, it lost so much of its weight, as to retain no more than 25 or 30 of oxigen to 100 of iron *. Part of the oxigen said to be expelled by heat by Lavoisier.

It is difficult to place confidence in this result of Lavoisier: 1st, because there is no indication of his having taken the necessary precautions, to oxide completely the metal he dissolved: 2dly, because, according to the experiments of Berthollet and Fourcroy, the oxide does not part with any of its oxigen to return to the state of oxidule, to whatever temperature it be exposed. but this questionable.

It remains for us therefore to compare the two results of Bucholz and Gueniveau. As it is difficult for us to know with certainty, whether the oxide of Bucholz did not still contain some portion of oxidule mixed with it; and whether the iron, which he employed, were perfectly pure; every thing leads us to prefer the second result, which gives the larger proportion of oxigen. Both however having taken the Comparison of the other results.

* Mém. de l' Ac. des Sciences, 1782, p. 5.

same

same precautions, and employed the same care in their experiments, we will leave it for the present undecided, whether 100 of iron be mixed with 42 or 44 of oxigen in the red oxide.

The oxide precipitated by an alkali, by Guyton,

2nd *Method.* Guyton dissolved iron in nitric acid, and precipitated it with salt of tartar. The precipitate, after being repeatedly washed and dried, had increased 32·1 on 100 of iron.

and Lavoisier.

Lavoisier precipitated the nitric solution of iron by caustic alkali, and obtained two different precipitates. 1st, When the solution was weak, made without heat, and precipitated by pure caustic ammonia, this illustrious chemist had a black precipitate, in which the weight of oxigen to 100 of iron was 30. 2dly, When the acid was concentrated, and the solution promoted by heat, the iron received an increase of 40 or 50 per cent, whatever were the acid employed; and this precipitate, after being calcined, retained only 25 or 30 parts of oxigen to the 100 of iron.

Oxide of iron in sulphuric acid diluted with water, when precipitated from its solution, and afterward dried in close vessels, Lavoisier found to be in the state of ethiops, and containing from 25 to 30 parts of oxigen to 100 of metal.

Probable source of errour in Lavoisier's exp.

This great diminution of the weight of the precipitated oxides obtained by Lavoisier might lead us to suppose, that some coally substances had got into the vessel containing the oxide, and contributed to the reduction of part of the oxide to the metallic state.

These experiments inconclusive.

It is difficult to draw accurate conclusions from these experiments, because it is not certain, whether, by virtue of that chemical action of which Berthollet has proved the existence, a portion of the alkali had not been carried down by the precipitate, and a portion of the oxide retained by the menstruum. In fact Fourcroy found, that the oxides of iron precipitated by ammonia retained a portion of the precipitant.

Solution of silver precipitated by iron by Bergman.

3d *Method.* Bergman remarked, that 20 parts of steel of Osterby threw down 66 parts of silver dissolved in sulphuric acid; whence it follows, that 100 parts of silver would have been precipitated by 29 iron. From this Lavoisier concludes,

cludes, that 100 parts of iron would take up 37·29 of oxigen in this experiment.

The result here depends on too many circumstances, to make known the proportion of iron contained in the oxide or oxidule. In fact the quantity of precipitate compared with the iron dissolved depends, 1st, on the quantity of oxigen combined with the silver in the solution; 2dly, on the proportion of silver precipitated from nitric acid by mercury; 3dly, on the quantity of oxigen which mercury takes up on being dissolved in nitric acid: and if these three data be not strictly accurate, the final result cannot be determined with precision. Proust says he has discovered, that silver may be oxided either at a maximum or a minimum in nitrate of silver*; and hence we have a fresh source of errour in the inference of Lavoisier. Objections. Two nitrates of silver.

Bucholz attempted to determine the proportion of oxigen in the oxidule of iron, by supposing that in the oxide to be known. With this view he passed the vapour of water over red hot iron, and obtained an oxide, which on trituration furnished a black powder. This, being subjected to the action of muriatic acid, and to several other tests, was found to be an oxidule of iron. Bucholz formed the protoxide of iron by means of heat and water,

This oxidule was dissolved in nitric acid in the same manner as iron, the fluid evaporated, and the residum kept at a red heat for a quarter of an hour, when it had gained an addition of one tenth to its weight. This, on the supposition that the red oxide contains 42 of oxigen to 100 of iron, would give 30† of oxigen to 100 of iron for the oxidule; and on the supposition of the red oxide containing 44, the oxidule would contain 31 to 100 parts of metal; and 100 of oxide [oxidule] would be composed of 76·3 iron, and 23·7 oxigen, which agrees with the results of Lavoisier and Bucholz. and converted it into peroxide by nitric acid. Calculations from this.

6th method. *Of the quantity of oxigen determined by the decomposition of nitre.*

Guyton projected equal parts of iron filings and nitre into a red hot crucible, washed the residuum repeatedly in boiling water, and obtained an oxide, in which the iron had received an addition of 34 parts to 100. Iron oxided by nitre, by Guyton,

* Journal de Phys 1799, vol II, p 221.
† 29 1 only. C.

Lavoisier

and Lavoisier, Lavoisier imagined, that the iron employed by Guyt was not pure, because, in the experiments he made, he o tained an increase of 45 to 100.

differed greatly. The difference between the results of Guyton and Lav sier is too great to allow us to deduce from these experime the proportions of oxigen contained in the oxide.

Conclusion from the experiments recited respecting the prop tions of oxigen and iron contained in the different oxides.

General conclusions. The numerous experiments on the oxidation of iron, th have been mentioned, and which were made by persons lebrated equally for their talents, sagacity, and precisi may be divided into two classes. The first, executed at time when the science was not sufficiently advanced, to i dicate all the causes that might influence the results, are n sufficiently accurate for us to make use of them. T second, made with the care and precision, that the prese state of the science admits, ought seemingly to enable us determine the proportions of oxigen and pure metal in t oxides, if not accurately, at least with so near an approxim tion to the truth, that we may employ them with advanta in those chemical analyses in which iron is present.

Red oxide. The proportion of oxigen in the red oxide of iron exhib some uncertainty. It was found to be 42 to 100 of iron Bucholz; 44 by Mr. Gueniveau; and 45 by Desroches a myself.

Several considerations lead me to adopt the proportion 45 of oxigen to 100 of iron, supposing the three expe ments, which give these different proportions, to have be made with the same degree of accuracy; according to wh the composition of 100 parts of red oxide would be in rou numbers 69 iron, and 31 oxigen.

69 iron to 31 oxigen.

Reasons for taking this proportion. Among the considerations that lead to the adoption this proportion, the following may be distinguished. 1 The method of oxidation by the air assisted by heat, wh is the most direct and simple. 2d, The difference betwe the results of Bucholz and Gueniveau, 44 being nearer 45 than to 42. 3d, The difference in specimens of iro Iron rendered impure, either by the union of carbon, oxidule remaining in it, or by scoriæ, would receive a small addit

addition of weight from the combinaton of oxigen, than iron in a purer state. Now as Bucholz did not analyse his iron separately, it is difficult to decide on his account. In the analysis there is an element very difficult to be determined in the present state of our knowledge, this is, the quantity of oxidule remaining. Now as, from these considerations, we should regard the highest result as nearest approaching the truth, this leads to the adoption of 45.

Black oxide.

The proportion of oxigen in the black oxide appears to vary between 30 and 31·8 of oxigen to 100 of iron. It is 30 in reducing the black oxides of the valley of Aost: it is 40 in reducing the black oxide of the isle of Elba with hidrogen gas. On dissolving the black oxide in nitric acid, the oxidule increases 10 per cent in passing to the state of red oxide. From this experiment it is inferred, that the black oxide contains 30 of oxigen to 100 of iron, supposing the red oxide to contain but 42 of oxigen. If, however, this oxide be estimated at 45, agreeably to our experiments, the black oxide will contain 31·8 to 100 of iron. Lastly, it amounted to 31·38 in the experiment where Lavoisier oxided iron by exposing it red-hot to the vapour of water in a copper tube; and to 30·38 according to Bucholz.

Reasons for taking the higher estimation,

There appears some difficulty in choosing between the results of 30 and 31·8 of oxigen to 100 of iron in the oxidules, yet we think the higher ought to be taken, because, 1st, the proportion derived from the oxidulated ores of the valley of Aost is liable to various uncertainties, respecting the identity of the mineral fused and the mineral analysed: 2dly, carbon always combines with the regulus, and it is difficult to determine its quantity precisely: 3dly, it seems more proper to calculate the difference of proportion between the black oxide and the red, by taking the latter at 45 than at 42: 4thly, of all the experiments, in which the black oxide has been obtained, that where the vapour of water was employed is the most direct: 5thly, considering the impurity of iron, the highest term of oxidation is always to be preferred, when its state is well determined.

From all these considerations we shall presume, that the black oxide is composed of 31·8 oxigen to 100 of iron; or 76 iron to 24 oxigen.

whence it will follow, that 100 parts of black oxide of iron are composed in round numbers of 76 iron and 24 oxigen.

White oxide. No method of obtaining the white oxide of iron is yet known, but that of dissolving the metal in an acid. When the oxide thus obtained is precipitated, it immediately combines with oxigen to generate black oxide, and frequently red oxide; so that we have no method of finding the proportion of oxigen in white oxide but by calculating from the hidrogen gas evolved during the solution.

In this way we found the proportion of oxigen to vary between 26·6 and 30. To choose between these two numbers taken from the experiments of Vauquelin, we should know the state of the iron dissolved, and the mode in which the solution was conducted. Calculating from the different degrees of impurity in iron, we shall be led to adopt the proportion of 30; yet, I think, it would be preferable to take that of 29 of oxigen to 100 of iron, deduced from the experiments of the French academicians, Monge, Vandermonde and Berthollet: because, 1st, it was the highest product they obtained from 21 varieties of good iron, which they dissolved: 2dly, they were particularly careful in measuring the gasses they obtained; 3dly, they reduced these measures to a uniform temperature and pressure, whence it was easy to calculate the exact quantity of oxigen corresponding; 4thly, in the experiment of Vauquelin, which may be considered as very accurate, the omission of the temperature and pressure, at which the gasses were obtained, renders us unable to compute exactly the quantity of oxigen.

77·5 iron to 22 5 oxigen. From these considerations it follows, that the white oxide is composed of very nearly 29 parts of oxigen to 100 of iron and that 100 parts of white oxide contain 77·5 iron and 22·5 oxigen.

Addition.

Difference between the native oxides of Aost and Elba. Mineralogists know, that there are very characteristic differences between the oxidule of iron of the valley of Aost, which the celebrated Haüy designates under the name of oxidulated iron, and the oxidule of the isle of Elba, which he calls oligist iron. The primitive form of the first is the octaedron and it yields a black powder; the primitive form of the second a rhomboid, somewhat acute, and it yields a red powder.

These two oxidules appear to differ in the former being only

only a pure oxidule, similar to that obtained from the oxidation of iron by water, the decomposition of which is assisted by heat, and consisting of 0·76 iron to 0·24 of oxigen; and the latter being a compound of red oxide and black oxidule of iron, in which the proportions of these two substances are variable.

The powders of oxidulated iron, and of oxidule of iron obtained by heat and water, appear to be identical. We have analysed several specimens of ore from the island of Elba, and found in every one very variable proportions of the two oxides. Of the two varieties mentioned in the preceding observations, one contained 33, the other 40 of oxigen to 100 of iron. Some others, that we analysed both in the moist and dry way, contained 35, 36, and 38 of oxigen to 100 of iron. One alone yielded us only 30, like the ore of the valley of Aost: but we had reduced and analysed separately two fragments taken from different parts of the same specimen.

Among the fine crystals of the isle of Elba a great deal of red oxide of iron is found; and it is principally in this state, that the ore is extracted for the smelting houses. In the mines of the valley of Aost, on the contrary, the oxide is much more rare, and the ore is pretty commonly extracted for the works in the state of oxidule.

This addition I give only as a sketch, that may assist the mineralogist in his researches, and facilitate the discovery of the causes of the differences presented by these two species of iron ore, which the learned Haüy had already divined with that sagacity, which he employs in all his labours.

These merely hints to mineralogists.

V.

On the Parts of Trees primarly impaired by Age. In a Letter from T. A. KNIGHT, *Esq. F. R. S. to the Rt. Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S.*[*]

MY DEAR SIR,

IN the first communication I had the honour to address to you, (it was in the year 1795,) I stated the result of many

Grafts last but for a limited period.

[*] Phil Trans. for 1810, p. 178.

 experiments

experiments on grafted trees, from which I inferred, that each variety can be propagated with success during a limited period only; and that the graft, or other detached part of an old tree, or old variety, can never form that, which can with propriety be called a young tree.

What organ first fails?

I have subsequently endeavoured to ascertain which, among the various organs that compose a tree, first fails to execute its office, and thus tends to bring on the incurable debility of old age; and the result of the experiments appears sufficiently interesting, to induce me to communicate an account of them.

Analogy between vegetable and animal organs.

Whatever difference exists between the functions of animal and vegetable life, there is a very obvious analogy between some of the organs of plants, and those of animals; and it does not appear very improbable, that the correspondent organ, in each, may first fail to execute its office; and satisfactory evidence of the imperfect action of any particular organ can much more easily be obtained in the vegetable than in the animal world. For a tree may be composed, by the art of the grafter, of the detached parts of many others; and the defective, or efficient, operation, of each organ, may thus be observed with the greatest accuracy. But such observations cannot be made upon animals; because the operations necessary cannot be performed; and therefore, though there would be much danger of errour in incautiously transferring the phenomena of one class of organized beings to another, I conceive, that experiments on plants may be, in some cases, useful to the investigator of the animal economy. They may direct him in his pursuits, and possibly facilitate his inquiries into the immediate causes of the decay of animal strength and life; and on a subject of so much importance to mankind, no source of information should remain unexplored, and no lights, however feeble, be disregarded.

Experiments may be made on trees that cannot on animals.

Organs of vegetables and animals compared.

Naturalists, both of ancient and modern times, have considered the structure of plants as an inversion of that of animals; and have compared the roots to the intestines, and the leaves to the lungs, of animals; and the analogy between the vegetable sap and animal blood is very close and obvious. The experiments also, of which I have at different

different periods communicated accounts to you, supported by the facts previously ascertained by other naturalists, scarcely leave any reasonable grounds of doubt, that the sap of trees circulates, as far as is apparently necessary to, or consistent with their state of existence and growth.

No deficiency in the roots occasions the decay of trees.

The roots of trees, particularly those in coppices, which are felled at stated periods, continue so long to produce, and feed, a succession of branches, that no experiments were wanted to satisfy me, that it is not any defective action of the root, which occasions the debility and diseases of old varieties of the apple and pear tree; and indeed experience every where shows, that a young seedling stock does not give the character of youth to the inserted bud or graft. I, however, procured plants from cuttings of some very old varieties of the apple, which readily emit roots; and these plants at the end of two years were grafted, about two inches above the ground, with a new and very luxuriant variety of the same species. These grafts grew very freely, and the roots themselves, at the end of four or five years, probably contained at least ten times as much alburnum, as they would have contained, had the trees remained ungrafted. The roots were also free from any appearance of disease, or defect.

Plants from cuttings of old varieties of the apple grafted.

Golden pippin grafted on the crab, and a crab on the graft of the pippin,

Some crab-stocks were at the same time grafted with the golden pippin, in a soil where the wood of that variety rarely lived more than two years: and I again grafted the annual shoots of the golden pippin with cuttings of a young and healthy crab-tree, so as to include a portion of the wood of the golden pippin between the roots and branches of the native uncultivated species, or crab tree; and in this situation it grew just as well as the wood of the stock and branches. Some branches also of the golden pippin trees, which I mentioned in my former communication of 1795, being much cankered, were cut off a foot above the junction of the grafts to the stocks; and were regrafted with a new and healthy variety. Parts of the wood of the golden pippin, in which were many cankered spots, were thus placed between the newly inserted grafts, and the stocks; and these parts have subsequently become perfectly free from disease, and the wounds, previously made

with other experiments,

made by canker, have been wholly covered with new and healthy bark. These facts, therefore, satisfied me, that the debility and diseases of old varieties of fruits of this species did not originate in any defective action of the bark or alburnum, either of the root, or of the stem and branches, and my attention was consequently directed to the leaf and succulent annual shoot.

showed, that there was no defective action of the bark or alburnum.

A few crab-stocks were grafted with cuttings of the golden pippin, in a situation and soil, where I had previously ascertained, that the wood of the golden pippin rarely remained in health at the end of a second year; and, as soon as the annual shoots had acquired sufficient growth and firmness, numerous buds of a new and luxuriant variety of apple, which had recently sprung from seed, were inserted in them. During the succeeding winter, the natural buds of the golden pippin branches were destroyed, and those inserted suffered alone to remain: and as soon as the leaves of these had unfolded, and entered on their office, every symptom of debility and disease disappeared in the bark and wood of the golden pippin: and each continued to perform its office, just as well as the wood and bark of the young seedling stocks could have done under similar circumstances. I made nearly the same experiments on the pear tree, and with the same result.

Experiments on the leaves and annual shoots.

I have endeavoured, in several former communications, to prove, that the sap of plants circulates through their leaves, as the blood of animals circulates through their lungs; and I have not subsequently found any facts, in the writings of other naturalists, or in my own experiments, which militate against this conclusion. I have also observed, that grafted trees, of old and debilitated varieties of fruit, became most diseased in rich soils, and when grafted on stocks of the most vigorous growth; which has induced me to suspect, that in such cases more food is collected, and carried up into the plant, than its leaves can prepare and assimilate, and that the matter thus collected, which would have promoted the health and growth in a vigorous variety, accumulates, and generates disease in the extremities of the branches and annual shoots, while the lower part of the trunk and roots remain, generally, free from any apparent disease.

The sap circulates through the leaves, as the blood through the lungs.

disease. I am, therefore, much disposed to attribute the diseases and debility of old age in trees to an inability to produce leaves, which can efficiently execute their natural office: and to some consequent imperfection in the circulating fluid. It is true, that the leaves are annually reproduced, and therefore annually new: but there is, I conceive, a very essential difference between the new leaves of an old, and of a young variety: and in support of this opinion, I shall observe, that the external character of the leaf of the same variety at two, and at twenty years old, is very dissimilar; and it therefore appears not improbable, that farther changes will have taken place at the end of two centuries*.

The leaves the organs that first fail.

If these opinions be well founded, and the leaves of trees be analogous to the lungs of animals, is it very improbable, that the natural debility or old age of trees and of animals may originate from a similar source?—This is a question, upon which I am not by any means prepared to give an opinion: but I believe it will very generally be admitted, that the human subject is best formed for long life, when the chest is best formed to permit the lungs to move with most freedom. I have also long and attentively observed among our domesticated animals, that those individuals longest retain their health and strength, and best bear excessive labour and insufficient food, in which the chest is most deep and capacious, proportionately to the length of current the circulating fluid has to run; and the same remark will, I believe, be generally found applicable to the human species.

Longevity principally affected by the state of the lungs?

I am, my dear Sir,
with great respect, sincerely yours,
THO. AND. KNIGHT.

Downton, Feb. 26, 1810.

* The leaf of a seedling apple or pear-tree, when the plant is very young, is generally almost wholly free from the pubescence or down, which subsequently appears on its under surface; and which Bonnet and Mr. Mirbel have supposed to increase its surface and powers. But I feel little disposed to adopt this hypothesis, having observed, that the leaves of some new varieties of the apple, which have sprung from seeds of the Siberian crab, have both surfaces nearly equally smooth; and that these varieties grow faster, and bear heavier crops of very rich fruit, than any others, without being exhausted or injured.

Down on leaves a symptom of decline.

VI.

Remarks on Mr. DALTON'S *Hypothesis of the manner in which Bodies combine with each other. In a Letter from* JOHN BOSTOCK, *M. D.*

To Mr. NICHOLSON.

SIR,

New principle in chemistry.

IN the first part of Mr. Dalton's system of chemical philosophy, he announced the existence of a new principle, respecting the manner in which bodies combine with each other. At the time he went no farther, than merely to state the outline of his hypothesis, refering to the subsequent part of his work, for the facts and experiments from which it was derived. The second part of the system being now published, and a considerable portion of it being occupied with details, which may be considered as dependant upon, or illustrative of the hypothesis, and the truth of it being assumed as the foundation of a great part of Mr. Dalton's reasoning, we may be fairly entitled to enter into an examination of the proofs, which are adduced in its support.

Different modes of establishing a new hypothesis.

In order to assist us in forming a clear conception of the subject, it may be desirable to refer to the different ways in which we attempt to establish a new hypothesis. We may show, that it is consonant to the generally received laws of physics, that it coincides with previously admitted facts, and is deducible from them: or, proceeding without these previous steps, we may propose a new opinion, that is unsupported by analogy or induction, and afterwards endeavour to substantiate it, by experiments and observations made expressly for that purpose. To the latter of these the term *hypothesis* more strictly applies, while the former is more correctly denominated *theory*; and as our plan of proceeding in the investigation of any truth must be considerably different, according as we adopt the one or the other of these modes, it is of some importance to bear in mind the distinction, which subsists between them. It will, I think, be found, that Mr. Dalton's doctrine is altogether *hypothetical*, or one which depends for its proof entirely upon subsequent observations and experiments. By those who

who are in the habit of philosophical investigation, it will not be supposed, that any degradation of Mr. Dalton's doctrine is intended by this statement of what I conceive to be the correct view, in which it ought to be regarded; some of the most important discoveries of modern times have originated from mere hypothesis, nor does it appear that any objection can be urged against the use of it, provided only we bear in mind the nature of the ground on which it is founded, and recollect that its proof depends entirely upon the strength of the facts that are adduced in its favour.

Remarks on Mr. Dalton's terms.

I shall offer a few remarks upon the terms that are employed by Mr. Dalton, and first upon the word *atom*, which holds so prominent a place in his pages. Considerable merit is due to him for the clear manner in which this part of the subject is brought into view, and this, I conceive, is in no small degree owing to his having employed a term, the meaning of which was established both by custom and analogy, so that it could scarcely be misunderstood *, whereas the words *particle* †, generally employed by the English chemists, and still more, *molecule* ‡, employed by the French, were obliged to be altered and retracted from their popular acceptation, before they could be employed in a scientific discussion. To the word *atom* no objection can attach. I cannot, however, assent to the manner in which Mr. Dalton has used the words *binary*, *ternary*, &c.; because, I think, it is calculated to introduce confusion into chemical language. The words themselves are modern, they are confined to the writing of the chemist, and were formed to express a peculiar constitution of bodies, yet Mr. Dalton employs the terms in a sense essentially different from that originally imposed upon them. I am not fond of proposing new terms, being aware of the difficulty of inventing such as may be unexceptionable, yet I shall venture on the present occasion to suggest, that *binate*, *trinate*, &c. may serve to denote that the substance in question consists, of 2, 3, or more atoms, without the liability to errour,

Atom.

Binary, ternary, &c.

Other terms proposed for these.

* Ατομος, insecabilis, indivisibilis.

† Diminutive of pars, a little part.

‡ Diminutive of moles, a little mass.

that

that is involved in the words *binary*, *ternary*, &c., which refer, not to the quantity, but to the quality of the atoms*.

After these preliminary observations, I shall proceed to the subject more immediately under consideration. In the conclusion of the first part of his system, Mr. Dalton introduces a short chapter on chemical synthesis, in which he informs us, that "one great object of his work is "to show the importance and advantage of ascertaining the "relative weights of the ultimate particles, both of simple "and compound bodies, the number of simple elementary "particles which constitute one compound particle, and the "number of less compound particles which enter into the "formation of one more compound particle." He then goes on to state, that when two bodies A and B are disposed

Chemical synthesis.

* I hope I shall not be thought hypercritical, if I point out an inaccuracy into which you have fallen, in your translation of Mr. Grotthuss's paper on metallic arborization, inserted in your last number. The following sentence occurs in it;" "Let us suppose a thread of water formed "of three integrant particles, each composed of two atoms of oxigen, "represented by 8, and one of hidrogen, &c." Knowing by the date of the paper, that between 3 or 4 years must have elapsed since it was written, I was surprised to meet with the word *atom* in this connexion, and was induced to turn to the original, where I found the expression to be; "Considérons un filet d' eau formé de trois molécules inte- "grantes dont chacune soit composée d' oxigène, representé par 8, & "d'hidrogène représenté par 1, &c." In works of science, particularly where the language has been controverted, we should closely adhere not only to the *sense*, but, where it is possible, to the *terms* of the original. Ann. Chim. V. LXIII, p. 19, 20. **

** [*Answer.* It appears to me, that one great source of the corruption of language is the retaining the words of one language in what is called a translation into another, without necessity; and that this has been of late years a growing evil in our own, partly from the carelessness and partly from the ignorance of translators, which I have generally been studious to avoid. To the word *molecule* I have always felt a particular objection, not only because I conceive our English word particle to be of the same signification, but because, when pronounced, its sound is liable to excite a ludicrous idea. Mr. Dalton's recent observation, that he did not understand the meaning of the word particle, induced me to employ that of atom, to which I did not imagine any objection could lie. C.]

to

to combine, their atoms must unite in the following order, beginning with the most simple case.

" 1 atom of A + 1 atom of B = 1 atom of C, binary.
" 1 atom of A + 2 atoms of B = 1 atom of D, ternary.
" 2 atoms of A + 1 atom of B = 1 atom of E, ternary.
" 1 atom of A + 3 atoms of B = 1 atom of F, quaternary,
" 3 atoms of A + 1 atom of B = 1 atom of G, quaternary, &c."

The author then proceeds to lay down some "general rules," which, not being immediately deducible from the foregoing statement, nor, as far as I can judge, having any previous analogies in their favour, we must regard as speculative positions, the proof of which is reserved for the 2nd part of the work. The first four of these rules are as follows. " 1st. When only one combination of two bodies can be " obtained, it must be presumed to be a *binary* one, unless " some cause appear to the contrary. 2nd. When two com- " binations are observed, they must be presumed to be a " *binary* and a *ternary*. 3d. When three combinations are " obtained, we may expect one to be *binary*, and the other " two *ternary*. 4th. When four combinations are observed, " we should expect one *binary*, two ternary, and one qua- " ternary, &c." In support of these positions Mr. Dalton offers no arguments, but resting their truth entirely upon their intrinsic probability, he proceeds, without any hesitation, to deduce from them the most important conclusions, and such as lie at the foundation of a great part of his subsequent reasoning.

Composition of water, and of ammonia.

From the first of the above rules Mr. Dalton draws the two conclusions, that water is a binary compound of hidrogen and oxigen, having the relative weight of its two elementary atoms nearly as 1 to 7; and that ammonia is a binary compound of azote and oxigen, the atoms of which have the weight of 1 to 5 respectively. But before we can follow Mr. Dalton through this train of consequences, there are some difficulties to be cleared up, which oppose our progress at the very outset. When bodies unite only in one proportion, whence do we learn that the combination must be binary? Why is it not as probable, that water is formed of two atoms of oxigen and one of hidrogen, of two atoms of hidrogen

Objections.

hidrogen and one of oxigen, or in short of any assignable number of atoms of hidrogen and oxigen? I do not perceive, that Mr. Dalton has given any reason in support of this binary combination, in preference to all the rest, and I am unable to conjecture what reason can be urged in its favour. If we feel a difficulty in assenting to the first of these rules, still more shall we be inclined to hesitate, as we advance to the more complex cases, which are stated in the 2nd, 3d, and 4th. It is obvious, that as there is more scope for variety in the mode of combination, in the same ratio does the probability of Mr. Dalton's supposition diminish. It is not necessary to enter into the consideration of the other "general conclusions," because the estimates which are formed of the weights of oxigen, hidrogen, and azote, as laid down in the first two, enter as elements into all the subsequent calculations. We may therefore conclude this part of the inquiry by stating, that the positions respecting the weight of the atoms of oxigen, hidrogen, and azote, as laid down in the first part of Mr. Dalton's system, are entirely hypothetical, are assumed without any authority either from analogy or induction, and that consequently, they must rest entirely upon such facts as the author brings forwards expressly for their support.

Assumptions of Mr. Dalton

The points which Mr. Dalton has here assumed, and which remain to be proved, are the following. First, that when only one combination of two elementary bodies can be obtained, it must be *binary*; 2nd. That only one combination of oxigen and hidrogen, and only one combination of hidrogen and azote can exist; 3d. That the ratios of 1 to 7, and of 1 to 5 express the relative weights of these atoms. It may be confidently asserted, that these steps in the proof are so intimately connected together, and that the last depends so entirely upon the previous ones, that the least flaw in either of them must utterly demolish the whole hypothesis. I shall now turn to the 2nd part of the system, where the author promised that the facts and experiments should be detailed, from which the conclusions were derived. The first chapter is on "elementary principles," oxigen, hidrogen, azote, &c.; each of these individually forms the subject of a short section, in which the mode of procuring them

Elementary principles.

is

is described, and an outline is given of their chemical and physical properties, but nothing occurs, either in the way of proof or illustration of the "general conclusions." We, however, naturally expect, that we shall find the information of which we are in search, when we come to the next chapter, which professedly treats of compounds of two elements, and first of water. After giving a brief account of the experiments on the composition and decomposition of water, experiments which have been repeated in various forms, by the most distinguished chemists of France and England, and respecting which the utmost pains have been taken to ensure accuracy, Mr. Dalton remarks, that "the "general result was that 85 parts by weight of oxi- "gen unite to 15 of hidrogen to form water." In this estimate, however, he does not acquiesce; he refers to some objections that have been urged against it by Mr. Humboldt and Gay-Lussac, whose reasoning on this subject he considers as "perfectly satisfactory." They conceive, that in consequence of the aqueous vapour which gasses usually contain, a less quantity of real hidrogen must have been employed, than was assumed in the calculations, and they reduce it from 15 parts to 14·3 parts, increasing, of course, the quantity of pure oxigen to 85·7. Mr. Dalton adds, "the relation of these numbers is that of 7 to 1 nearly." He continues; "There is another consideration "which seems to put this matter beyond doubt. In Vol- "ta's eudiometer, two measures of hidrogen require just "*one* of oxigen to saturate them. Now, the accurate expe- "riments of Cavendish and Lavoisier have shown, that "oxigen is nearly 14 times the weight of hidrogen; the ex- "act coincidence of this with the conclusion above deduced "is a sufficient confirmation." Notwithstanding the acknowledged sagacity of the author, I cannot but consider the above remarks as totally irrelevant to the question under discussion. The facts that are adduced refer merely to the relation which the bulk of hidrogenous gas bears to its specific gravity, and have no relation to either of the fundamental points in Mr. Dalton's hypothesis.

Compounds of two elements. Water.

Having now gone through the chapter in which the promised facts and experiments are contained, it may be proper to

The facts do not support

the assumptions.

to consider how far they substantiate and authorize the conclusions that have been deduced from them. Nothing, as I can judge, has been advanced, which has the least reference to the first position, that when only one combination of two bodies can be obtained it must be *binary*. This therefore, which is the foundation of all the argument, must be regarded as a mere postulate, unsupported by facts, and not resting immediately either on analogy or induction. The 2d position, that only one combination of oxigen and hidrogen, and only one of hidrogen and azote can exist, rests rather on a different foundation from the preceding. We have never yet been able to produce more than one combination with each of these substances, therefore Mr. Dalton concludes, that only one combination can possibly exist. But it is evident that this is not a legitimate way of reasoning; it amounts to no more than a presumption, and that drawn from our ignorance; and is in contradiction to the general analogy of chemical affinities*. From what has been stated I think it will be admitted, that Mr. Dalton's hypothesis fails entirely in its two fundamental positions, that one of them is entirely without support, and that the other is founded upon a presumption which scarcely amounts to a probability. In this state of the case we might perhaps be justified in discarding the hypothesis altogether, or at least in requiring the author to bring forward some direct arguments in its favour, before we should pay any farther attention to it. I shall, however, pursue a different course, and shall examine how far the consequences would fairly follow from the premises, supposing that these were demonstrated. Nor is this investigation to be considered as a matter of mere speculative curiosity, because, although the hy-

Are the consequences fairly deducible from the premises?

* Although in forming his general conclusions Mr. Dalton assumes, that only one combination of oxigen and hidrogen can exist, and from this circumstance argues that water is a binary compound, and hence deduces the weight of its atoms, *a calculation, upon which all the details of the hypothesis depend*, yet in the 2d part of his system he endeavours to show, that the fluoric, muriatic, oximuriatic, and hyperoxmuriatic acids are all compounds of oxigen and hydrogen, and even forms estimates of the number of atoms of each element which enters into their composition. So dangerous is it for the most acute mind to indulge in merely speculative reasoning!

pothesis

pothesis is at present unsupported by any evidence, yet it implies nothing contradictory to reason or experience; so that, if it appear that there are any number of facts which coincide with it, or that are conveniently explained by it, they must be considered as forming an indirect argument in its favour. Now if we admit the truth of the hypothesis, so far as respects the union of the two elements atom by atom, a necessary consequence is, that the compound will be either *binary**, or that the number of atoms in the substance which exists in the greatest quantity will be some multiple of the atoms existing in the smaller quantity. I will take the example of sulphur and oxigen. These bodies unite in three different proportions, of which one must be *binary*, i. e. 100 atoms of sulphur and 100 atoms of oxigen will unite and form 100 atoms of some compound, which compound we might a priori suppose would be that in which the strongest affinity is exerted between the two elements. In all the other compounds it necessarily follows, that the number of atoms which exist in the largest quantity, must be either as 200, 300, 400, or some multiple of 100; for it is obvious, that the substances could not combine atom by atom, were the greater number of atoms to exist in the ratio of 120, 150, or in any quantity intermediate between the centesimal numbers. It seems the most natural to regard the sulphuric acid as the *binary* compound of sulphur and oxigen, or that 100 atoms of sulphur and 100 atoms of oxigen form 100 atoms of sulphuric acid. According to the most accurate experiments, the respective weights of the component parts of sulphuric acid are 100 parts of sulphur to 136·5 parts of oxigen†, therefore the weight of an atom of sulphur will be to the weight of an atom of oxigen as 1 to 1·365, or nearly as 3 to 4. Let us now try how this supposition will coincide with the composition of the next compound of sulphur and oxigen, sulphurous acid, 100 parts of which are stated, upon the respectable authority of Dr.

Compounds of sulphur and oxigen.

* The word *binary* is here, and in all the other parts of this paper, employed in the sense which Mr. Dalton assigns to it; but it is obvious, how much confusion may be introduced by the circumstance of there being two different significations applied to the same scientific term.

† Klaproth, Gehlen's Jour. V. 109.

Thomson,

Compounds of sulphur and oxigen.

Thomson, to consist of 53 parts of sulphur and 47 of oxigen, or 100 parts of sulphur to 88·6 of oxigen*. Now in this case the proportion of oxigen to the sulphur is diminished, therefore the 100 parts of oxigen, which entered into the composition of sulphuric acid, will be combined with a greater number of atoms of sulphur, and the next smallest quantity with which they can unite is the next multiple of 100, that is 200. Sulphurous acid will therefore be composed of 100 atoms of oxigen, and 200 atoms of sulphur. Now supposing the weight of an atom of sulphur to be to an atom of oxigen as 1 to 1·365, the weight of the two atoms of sulphur in sulphurous acid will be to the single atom of oxigen as 2 to 1·365, nearly as 6 to 4, so that 100 parts of sulphurous acid will consist of 60 parts of sulphur and 40 parts of oxigen. The third compound of sulphur and oxigen is the oxide of sulphur, and this is stated, upon the same authority with the last†, to be composed of 100 parts of sulphur with 7 of oxigen. Here as before we are to take the oxigen as the fixed quantity, and to find some multiple of 100 which may express the proportion of sulphur. Now if we suppose that one atom of oxigen unites with 19 atoms of sulphur, to form the oxide of sulphur, or that 100 atoms of the oxide of sulphur are composed of 100 atoms of oxigen and 1900 atoms of sulphur, we shall find, by following the same method of calculation, that the weight of sulphur to that of oxigen will be very nearly as 57 to 4. With respect to sulphur therefore it appears, that one case is favourable to the hypothesis and the other adverse to it; for the analysis of the sulphurous acid makes the sulphur to the oxigen as 53 to 47, while the proportion of 57 to 4, for the oxide of sulphur, agrees very nearly with Dr. Thomson's estimate of 100 to 7‡.

* Thomson, I. 87. † Thomson, I. 88.

‡ As the oxide of sulphur has probably a real existence, and as the numbers which Dr. Thomson has assigned for its constituents coincide remarkably with Mr Dalton's hypothesis, I have given it a place in the calculations, although my opinion is, that we have not yet sufficient grounds for forming an exact estimate of the proportion of its elements, and the above coincidence is purely accidental. See Dr. Thomson's paper in Nich. VI, 101.

I will

I will now apply the same mode of examination to the compounds of oxigen and carbon, carbonic acid and carbonic oxide. According to the hypothesis, one of these must be binary; let us suppose that it is the carbonic acid which is in this state, or that 100 atoms of carbonic acid consist of 100 atoms of oxigen and 100 atoms of carbon. But the carbon in carbonic acid is to the oxigen as 28 to 72, or nearly as 100 to 257, therefore the atom of carbon will be to the atom of oxigen as 1 to 2·57. In the oxide of carbon the quantity of oxigen being diminished, we must take some multiple of 100, to express the ratios of the increased quantity of carbon. If we suppose that two atoms of carbon unite to one of oxigen, it will make the proportion 2 to 2·57. This will give the constituents of carbonic oxide to be 100 parts of carbon to 128·5 of oxigen, whereas, from the analysis of this substance we learn, that 100 parts of carbon require 146 parts of oxigen to convert them into carbonic oxide*. I have here assumed, that carbonic acid is the binary compound, but I will now reverse the supposition, and make the oxide of carbon to consist of 100 atoms of oxigen and 100 atoms of carbon. According to this principle the weight of the atom of carbon will be to the atom of oxigen as 1 to 1·46, and proceeding as above for the constituents of carbonic acid we shall have 100 atoms of the acid to consist of 100 atoms of carbon and 200 of oxigen, making a ratio nearly of 1 to 2·9, which give the constituents of carbonic acid to be 100 parts of carbon to 290 parts of oxigen, whereas we find by analysis, that 100 parts of carbon require only 257 parts of oxigen to be converted into carbonic acid†. Compounds of carbon and oxigen.

I will now examine in the same manner the compounds of oxigen and azote. I will, in the first place, assume that nitric acid is the binary compound, and as 100 parts of azote require 236 parts of oxigen to be converted into nitric acid‡, it follows that the atom of azote is to that of oxigen as 1 to 2·36. Nitric oxide receiving an addition of azote, the proportion will be as 2 to 2·36, which will make the constitution of the oxide to be as 100 to 118, whereas it is stated to Compounds of oxigen and nitrogen.

* Thomson, II, 150. † Thomson, II, 149. ‡ Thomson, II, 163.

be in the ratio of 100 to 136*. With respect to the nitrous oxide, the ratio seems favourable to the hypothesis, for if we suppose, that 100 atoms of oxigen are combined with 400 atoms of azote, to produce 100 atoms of nitrous oxide, the proportion of the constituents would be in the ratio of 100 to 59, which is very nearly what it appears to be from analysis†. Here, therefore, one case appears to be favourable, and the other adverse to the hypothesis.

I shall now reverse the above calculations, and assume that nitrous oxide is the binary compound, which Mr. Dalton supposes to be the case. On this principle the atom of oxigen will be to the atom of azote nearly as 100 to 170, or as 1 to 1·7. The constituents of nitric oxide will then be in the ratio of 100 to 117·6, and of nitric acid as 100 to nearly 233. I have only admitted three compounds of oxigen and azote, because, even although we suppose nitrous acid, and oxinitric acid to be substances that are always uniform in their composition, a supposition which is at least doubtful, it seems natural even on Mr. Dalton's hypothesis, to regard them as secondary compounds, formed by the union of a certain number of atoms of nitric acid with a certain number of atoms of azote, in the one case, and of oxigen in the other.

The facts do not bear out the hypothesis.

I shall not think it necessary to pursue this examination any farther, the examples which have been adduced are sufficient to establish the conclusion, that although the facts occasionally coincide with the hypothesis, they more frequently oppose it. It will perhaps be said, that these analyses are not correct, and I am far from denying that this may be the case, although I have purposely selected these, which appear to stand upon the best foundation. But here we must recollect the difference between a theory and an hypothesis: had it been shown by a previous train of reasoning, that Mr. Dalton's opinion was supported by any strong analogy, or that any poweful arguments could be adduced in its favour, we might then have been permitted to adjust the facts to the theory. As the question now stands, however, we are to prove the hypothesis solely from the facts, and therefore we are not to object to the facts merely be-

* Thomson, II, 168. † Ibid.

cause

cause they do not coincide with the hypothesis. Mr. Dalton must first prove, that the analyses are incorrect, and must rectify them; he must then compare them with the hypothesis, and if they generally coincide with it, he may adduce them as arguments in its favour. In the mean time, taking the facts as they now exist, I think we are warranted in concluding, that they are not favourable to the hypothesis.

Atoms may unite in various numbers.

After this examination of Mr. Dalton's arguments in support of his opinion, I shall conclude with some farther observations upon the same subject. In the first place, although it be admitted, that when bodies unite, a union must take place between the individual atoms of which they are composed, yet it does not follow, that this union must take place atom by atom, it does not follow, that one atom of A must unite with one atom of B, or with two atoms of B, &c.; I conceive it equally probable, that two atoms of A may unite with three atoms of B, four atoms of A with five atoms of B, in short, that any indefinite number of atoms of A may unite with any indefinite number of atoms of B. By this admission we should avoid one of the most embarrassing circumstances in Mr. Dalton's hypothesis, as it now exists, namely the necessity of the greater number of atoms being always some multiple of the less. This idea is perfectly compatible with the supposition, that a certain number of atoms of A have a greater tendency to unite with a certain number of atoms of B than with any other number, although it does not previously inform us what the ratio is between these two numbers.

Weight of component atoms not deducible from the proportions of a compound.

My next remark refers to the method, in which the weight of the atoms of two bodies is attempted to be estimated from the quantity of each of them that enters into their compounds. Admitting with Mr. Dalton, that a quantity of water consists of 7 parts of hidrogen and one of oxigen, are we to conclude that there are an equal number of atoms of these two substances, but that the atom of oxigen is 7 times heavier than the atom of hidrogen, or that there are 7 times as many atoms of the one kind as of the other? Mr. Dalton has adopted the former of these suppositions, whereas the latter appears to me the most probable. In forming an idea respecting the weight of an atom, we must

proceed upon the same principles as in other cases. Now what is generally assigned as the cause of the different specific gravities of bodies? Why is a cube of lead heavier than a cube of ivory? The most natural answer to this question is, that the particles of lead are nearer together, and therefore a greater number of them will be contained in any given space. Whether this difference in proximity be occasioned by interposed caloric, or any power of repulsion, independent of caloric, it is not our business to inquire. Now if we endeavour to form a conception of a single particle, or what has been denominated an atom, as a substance not made up of any smaller parts, and which cannot therefore be under the influence of any repulsive power, so far as its individual existence is concerned, it follows that atoms of all kinds must have the same specific gravity. If then they differ in their weight, there must be a corresponding difference in their size, an atom of oxigen for example must be 7 times as large as an atom of hidrogen; but were this the case it could scarcely be regarded as an atom, but as a substance of considerable extent, capable of being divided into still smaller parts.

Cause of specific gravity.

These observations, it is obvious, are entirely conjectural, and I have only brought them forward, to meet conjectures on the other side of the question. My object in this paper has been to point out the insufficiency of Mr. Dalton's hypothesis, without attempting to substitute another in its place; for important as the object is which he has endeavoured to accomplish, and ingenious as the attempt may appear, I am decidedly of opinion, that we have not yet a sufficient basis of facts, on which to erect so capacious a superstructure.

I am, Sir, yours &c.

J. BOSTOCK.

Knot'shole Bank, near Liverpool,
Feb. 23d. 1811,

VII.

VII.

Effect of Changes in the State of the Atmosphere on Mr. DE LUC'S *Electric Column. Communicated by* THOMAS FORSTER, *Esq.*

To Mr. NICHOLSON.

SIR,

I TAKE the liberty, with your permission, of communicating to the public, through the channel of your Journal, a curious coincidence. The electric column of Mr. De Luc, having been described in your pages, is of course well known to your readers. The two bells attached to the *plus* and *minus* end of the column of an instrument of this kind, kept by a relation of mine at Walthamstow, after having continued ringing (in the manner which has been described in your journal) for several months, began on the 4th of September last to pulsate very irregularly, and at long intervals. This irregularity continued through the month of September, and until the 16th of October, when they ceased to ring entirely. During the period of their hurried and irregular pulsation, that is, from the 4th of September to the 16th of October, a very peculiar kind of weather prevailed, strong easterly winds, very clear nights abounding with small meteors commonly called falling stars, the hygrometer rendered useless by the excessive dryness of the air, and the varied and multiform appearances of the *cirrhus* and *cirrho-stratus* clouds, (not followed by rain, as usual) marked a very remarkable state of the atmosphere. On the 16th of October, the day on which the bells stopped ringing, a remarkable change of weather became very evident, a strong current of air from the South, and the prevalence of the *spotted cirrho-stratus* cloud indicated the approach of rain, which ensued during the night, and rainy weather for several weeks succeeded. About a week ago the same kind of weather, which prevailed between the 4th of September and the 16th of October last, returned, the air became extremely dry, the wind became easterly, and very strong, the small meteors have been again observed at night, and the same kind

Connection of the state of the atmosphere with the action of Mr. De Luc's electric column.

kind of *cirrhus cloud* has prevailed, indicating a great disturbance of the electric state of the atmosphere; *and the bells, after having pulsated very irregularly for a week, have again completely stopped.* Thus there seems to be a connection between the *electric* state of the air, (indicated by the abovementioned circumstances) and the phenomena exhibited by Mr. De Luc's column, which is a circumstance to which Mr. De Luc wished to direct the attention of meteorologists, which has induced me to request the insertion of these observations in your interesting journal.

I remain, sir, yours, &c.

Clapton, Hackney, March 18, 1811. THOMAS FORSTER.

March 19.

P. S. It is remarkable, that the weather has completely changed again to day: on getting up this morning, I observed the *cirrho cumulus* cloud through the mist which covered the ground, the hygrometer indicated an increased moisture of the air; and *the bells of Mr. De Luc's column again ring with a regular but weak pulsation.*

VIII.

On the Nature of Oximuriatic Acid, in Reply to Mr. JOHN DAVY. *By* J. MURRAY, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR, *Edinburgh, March* 11*th*, 1811.

I SHALL beg your permission to occupy a few pages of your Journal with some remarks on the reply of Mr. J. Davy, in your last number, to the observations which I had offered on Mr. Davy's opinion of the nature of muriatic and oximuriatic acids.

Theory and hypothesis confounded.

T.. gentleman first remarks, that I have not attended to the distinction between theory and hypothesis, but have made promiscuous use of the two words, and have hence taken

taken an incorrect view of his brother's opinion, which, he adds, is a theory, not an hypothesis.

General use of the word theory.

The free signification generally given to the word theory in chemical language is so well understood, that I did not suppose it could have given rise to any ambiguity requiring to be pointed out. Theory, strictly understood, implies, no doubt, a principle established by just induction from individual facts, and applied to the explanation of phenomena; while in an hypothesis a principle is assumed, whence phenomena are attempted to be explained. But there is an intermediate kind of reasoning or speculation, in which there is partly generalization, partly hypothetical assumption—a principle being professedly inferred by induction, but requiring the aid of hypothesis to apply it to all the phenomena connected with the subject. To this the terms opinion, theory, and hypothesis are often indiscriminately applied; and theory in chemical language has more frequently this signification than any other. The successive revolutions in chemistry would lead us to doubt indeed if it can be often justly used in its more strict sense, implying a perfect induction, the certainty of which subsequent discoveries cannot change. The theory of Stahl, as it has been named, which at one period commanded universal assent, was quickly subverted. Much of the theory of Lavoisier, ample and conclusive as the evidence appeared to be on which it is founded, must, if late speculations be just, share the same fate. And even those more partial inductions, which appeared to have the utmost certainty, are many of them, it now appears, doubtful. To a philosophic inquirer this may perhaps suggest some caution in applying the term theory in its strict signification, and it may guard him against the most common of all errours—an undue confidence in our speculations, and the belief that the opinions of our day are demonstrated truths.

Source of Mr. J. Davy's mistake.

Mr. J. Davy's mistake, and which I should have obviated in my former paper, had I supposed any one attending particularly to the subject, and accustomed to scientific deduction, could have fallen into it, and had I not wished to avoid holding out unnecessarily Mr. H. Davy's opinion as purely hypothetical,

hypothetical, is the supposing this opinion to be a theory in the strict sense of the term. In stating the grounds of this opinion, he exposes too very clearly the source of his errour. Mr. Davy, he remarks, "combines oximuriatic acid gas with hidrogen gas, and forms muriatic acid gas. In his theory, muriatic acid gas is a compound of oximuriatic gas and hidrogen. He combines oximuriatic gas with sulphur, phosphorus, and the metals; and in his theory the resulting substances are compounds of the inflammable and metallic bodies respectively, and oximuriatic gas. Here we perceive no supposition, but a simple expression of facts, and this I humbly conceive is pure and genuine theory." It is of main importance Mr. Davy also remarks, "that expression of facts be not misrepresented. It is of great consequence that things be not termed notions, that theory be not considered as speculation."

Simple enunciation of the facts.

The proper expression of the above facts is, that from the mutual action of oximuriatic gas and hidrogen, muriatic acid gas is obtained, that from the mutal action of oximuriatic gas and inflammables, or metals, such and such substances are formed. That muriatic acid is a compound of oximuriatic gas with hidrogen, or that these substances are compounds of oximuriatic gas with the respective metals or inflammables, are inferences, which may be true, or may be false. They appear no doubt to be the most direct inferences, but the most obvious and direct conclusion from an experiment may not always be the just one. The shortest way of convincing Mr. J. Davy of this will be to state to him in the very form of expression, which he employs in the above quotation, conclusions which he cannot admit. I combine, I may say, oxide of mercury and muriatic acid, and form calomel. I conclude therefore, that calomel is a compound of muriatic acid and oxide of mercury. I combine muriatic acid and potash, and by dissipation of the water I obtain a solid product, which I consider as a compound of the muriatic acid and potash: and I perceive in these conclusions no supposition, but a simple expression of facts. Mr. Davy will in this however soon correct me, and inform me not merely that they are suppositions, but conclusions altogether false. He will perceive therefore, that the

Inferences deduced from these may or may not be true.

inference

inference which appears most obvious and direct is not always just, that it is sometimes necessary to take a more extensive or deep view, and will perhaps be convinced, that he has erred in the notion he has formed of the kind of induction, which constitutes a genuine theory.

Error from taking into consideration only part of the facts.

The cause of errours such as this is, and which has long been recognized as the fertile source of false speculations in physics, is the taking into view only part of the facts which belong to the subject—those which appear most favourable to our induction, instead of taking the whole into consideration, and from this general view forming the most probable conclusion. If the facts above quoted from Mr. Davy were all that are connected with the question, his conclusions might appear to be just. But there are others equally connected with it, to explain which, various hypothetical assumptions must be made, the probability of which ought to be considered, and the whole compared with any other induction that may be formed.

Formation of oximuriatic acid from muriatic acid and oxide of manganese.

Thus in distilling muriatic acid from black oxide of manganese, the oxide loses a portion of its oxigen, the muriatic acid disappears, and oximuriatic acid is obtained. The direct conclusion from this experiment (and it is equally direct with Mr. Davy's conclusion from the experiment of detonating oximuriatic gas and hidrogen) is, that the oxigen of the oxide has combined with the muriatic acid, and formed the oximuriatic; and to obviate this he is obliged to *suppose*, that the oxigen of the oxide combines with the hidrogen of the acid, forming water, and setting free the oximuriatic acid. If we expose liquid oximuriatic acid to solar light, oxigen is expelled, and muriatic acid remains; and the most direct inference from this is, that oximuriatic acid is a compound of muriatic acid and oxigen. Mr. Davy must have recourse to a less obvious explanation, and *suppose* a portion of water to be decomposed, its oxigen disengaged, and its hidrogen combined with the oximuriatic acid. And he has no proof of the formation and decomposition of water in these experiments, further than that they must take place, if his theory be true.

Both explanations hypothetical

If we consider therefore these two opinions under different aspects, if we set out as it were from different points, each

Both explanations hypothetical.

each will appear an induction, which to apply to all the phenomena farther requires some hypothetical assumptions. Considering the experiment of the production of oximuriatic acid, by distilling muriatic acid from substances which impart oxigen, the most direct conclusion is, that it is a compound of oxigen and muriatic acid, but to apply this in explaining the agencies of both acids, it is necessary to suppose in some cases changes to occur: the formation or decomposition of water for example, of which we have no independent proof. Considering on the other hand the experiment of the mutual action of oximuriatic and hidrogen gasses, the most direct inference is, that muriatic acid is a compound of these substances; but in adopting this as the basis of a theory, it is equally necessary to advance suppositions, and in particular to suppose without any actual proof, that in different cases water is formed or decomposed. These suppositions are not always required in the two systems in relation to the same fact; but still, wherever an hypothesis is required in the one, a corresponding hypothesis will be found necessary in some part of the other. They occur on the whole just as frequently in Mr. Davy's system as in the opposite one, and they are alittle supported by actual proof. He, for example, has no better proof, that water is formed when muriatic acid is distilled from black oxide of manganese, than I have of its formation in the mutual action of oximuriatic gas and hidrogen, or that it is decomposed when oximuriatic acid is exposed to solar light, than I have of its decomposition when a metal is acted on by muriatic acid gas. The two opinions are perfectly alike with regard to the evidence on which they rest derived from the above facts; and to select in one of them that part where the induction appears direct, and take it for granted that is true, the hypothetical assumptions, which must be farther made, necessarily follow: but to pursue the reverse method with regard to the other, to represent it as an hypothesis, by bringing forward the parts which require the assumption of hypothesis, and neglect or reject the more direct induction, is a mere sophism. This is exactly what Mr. J. Davy does, no doubt without being aware of it, his errour arising

arising from the confined and partial view he has taken of the subject.

All this is so obvious, that some apology is due to your readers for having illustrated it at any length. This has however been rendered necessary by the tone this gentleman has assumed through the whole of his observations. He has held out the opinion of his brother as a genuine theory resting on indubitable evidence, and has thought himself at liberty to represent the reasoning I have employed as mere speculation, and the explanations I have given as uniformly hypothetical, and having on this account no pretensions to be put in contrast with the others. He will now perceive, that the subject may be presented under a different light; and he will not perhaps again hazard the assertion, that his brother's "conclusions are not tainted by the slightest admixture of hypothesis." Least he should, permit me to add one or two illustrations, which are besides not unconnected with the subject.

Decomposition of nitrate of mercury by muriate of soda.

On adding to nitrate of mercury, muriate of soda, nitrate of soda and calomel are formed; chemists have therefore been accustomed to conclude by rules of evidence, which they thought sufficiently certain, that the nitric acid combines with the soda, and that the muriatic acid combines with the mercurial oxide, forming the calomel. According to Mr. Davy's opinion however, though the nitric acid unites with the soda, the muriatic acid does not unite with the oxide, but these substances decompose each other, the oxigen of the oxide unites with the hidrogen of the acid and forms water, and the calomel is a compound of oximuriatic acid and metallic quicksilver. Again, on adding muriatic acid to potash we form muriate of potash, and on exposing this product to heat, so as to obtain it dry, we have hitherto believed, apparently on very strict induction, that the water was expelled, and that the dry product is a compound of muriatic acid and potash. But in Mr. Davy's new system it is supposed, that the acid and the potash decompose each other, the oxigen of the one and the hidrogen of the other combine and form water, leaving a compound of oximuriatic acid and potassium. On dissolving this in water new changes occur, the water is decomposed, the potassium receives

Muriatic acid and potash.

receives oxigen, the oximuriatic acid, hidrogen, and a compound of muriatic acid and potash is again formed. Who does not perceive in all this abundance of hypothetical assumptions? asumptions I have no hesitation in saying more gratuitous and more complicated, than any required in the opposite system, or perhaps in any other chemical speculation.

Comparison of the two hypothesis.

Mr. Davy's opinion then I regard as an hypothesis: the opinion I have maintained I have also distinctly admitted in my former paper to be an hypothesis: each rests on an apparently probable induction, and is capable of being applied with more or less probability to the various phenomena. Placing them on this ground I considered and still consider the common doctrine as superior in simplicity, in requiring less strained and less complicated assumptions, in affording explanations of facts which the other does not explain, and in according with the general system of chemical theory; receiving therefore all the support which the evidence on which that theory rests can give, while the other is anomalous, and has the weight of that evidence against it.

I proceed to offer a few observations on some of the more particular topics of this discussion.

Real acids spoken of though not exhibited separately.

Mr. Davy remarks, that when I speak of muriatic acid I do not mean the whole ponderable part of it; water I consider as necessary to its existence in its gaseous state, and that it is to the substance free from this water that the term muriatic acid is applied. This is my meaning. But when he proceeds to the interrogations, has Mr. M. examined it in its insulated state? has he described its properties? or has any chemist ever obtained it? And when from the negative given of course to these he concludes, that I must regard muriatic acid gas as a compound of an unknown basis and water, he raises or magnifies difficulties of little importance, or at least not peculiar to this investigation. When I speak of real sulphuric or nitric acid, I mean the acid free from water, though it may not have been obtained in that state; and in thus using language which is familiar to chemists I am not aware, that I introduce any novelty of hypothesis. The real muriatic acid I consider as an acid from having reason to conclude, that it exists in its solid compounds,

pounds, neutralizing the bases with which it is in combination; in the same manner as I consider real sulphuric or nitric acid as an acid from knowing, that they exist in combinations free or nearly so from water, exerting a similar neutralizing power. Mr. Davy may if he pleases suppose sulphuric or nitrid acid to be a compound of an unknown basis and water, and conclude, that the supposition of the existence of these acids free from water is "speculation in the strictest sense of the word." But no chemist will find any difficulty in this. The apparently greater influence of water on the chemical powers of muriatic acid than the other acids probably arises from the facts with regard to the latter being more fully developed. On this subject I am at present engaged in an experimental investigation.

Improbability of two acidifying principles.

Mr. Davy proceeds to examine the objections which I had offered to his brother's opinion. To the remark, that there is some improbability in the hypothesis of two acidifying principles, he replies, that the only legitimate mode of reasoning is induction; and that we are not to judge of Mr. Davy's views by considering their probability or improbability a priori, but by a reference to facts. The observation would be just, were Mr. Davy's opinion a theory; but while it is an hypothesis the objection retains its force, for of two hypotheses proposed on any subject, it is fair in contrasting them to bring into view the probability a priori of the principle of the one compared with that of the other. The concession that he makes, that there is properly no acidifying principle, is rather more in point, and I have remarked in my Elements of Chemistry, in giving a view of the theory of acidity, that the supposition of acidity being a property rather derived from the base of acids than from their common principle is most conformable to Mr. Davy's speculations. Still the similarity of oximuriatic acid to oxigen so as to class them as acidifying principles is very remote; for allowing it to combine directly with any substance, it forms no acid in the common definition of the term, or any substance analogous to acids, but in its supposed combination with hidrogen, the subject of dispute; and the acidity of this, and its analogy to the common acids, are indeed such as to afford on the other hand as striking an

Perhaps not.

anomaly

anomaly in comparison with its other supposed combinations.

Want of action between charcoal and oximuriatic acid.

To the explanation I had given of the want of action between charcoal and oximuriatic acid, that it is owing partly to the absence of the water required in the constitution of muriatic acid gas, and partly to the absence of that disposing affinity exerted by the acid in establishing other combinations, Mr. Davy replies, that it would be less unobjectionable were it less hypothetical; that I do not prove muriatic and carbonic gasses contain water, I only suppose it—To this mistake that the one opinion is a theory, the other an hypothesis, I have been forced to recur so often, that it is scarcely necessary to point it out. Both are hypotheses, and if I can explain a fact by the principle of the one, which admits of no explanation by the principle of the other, the superiority of the former is with regard to this point sufficiently established—Besides, the application of the doctrine of disposing affinity alone affords an explanation of this fact. I consider it therefore as still giving a superiority to the common system, for nothing can appear more anomalous than that of all inflammable and metallic substances none but charcoal remains unchanged by oximuriatic acid, and nothing can be more satisfactory than to have a cause assigned for this. Mr. Davy considers it as of little importance, though it is this apparent anomaly, and the supposed difficulty of accounting for it according to the old opinion, which gave rise to the new doctrine, or at least first suggested the suspicion, that oximuriatic acid does not contain oxigen; he is satisfied with the observation, that we have no right to expect from a theory the explanation of ultimate facts; that is, not of comprehensive facts arrived at by a generalization which cannot be carried farther, for the fact in question is not of this kind, but of individual facts which the theory will not explain; a very safe conclusion, in which it may be proper for him to abide. This, he adds, is an ultimate fact, "one of those which constitute as it were the axioms of the science;" and he adds, that he is glad it is not tortured by hypothetical explanation. How it is elevated to an axiom, taking this word in its common sense of a fundamental proposition, self-evident, and therefore not admitting of

of demonstration, I do not understand; at least I do not perceive how this definition applies to the proposition, that oximuriatic acid will not combine with charcoal. Could Mr. Davy discover even a supposition by which this fact could be explained, I have little doubt that he would gladly remove it from the class of axioms, and from the facility with which he admits so many other suppositions, that he would submit it even to some degree of torture, to bend it into conformity with the system.

Action of water in favouring the evolution of carbonic acid.

On the fact with regard to the influence of water in favouring the disengagement of carbonic acid from bases with which it is combined, it is scarcely necessary to make any observation. I was aware, that it might operate by its affinity to the base, for I had suggested this some years ago, in the discussion of the question with regard to the influence of water on the constitution of elastic fluids, in my chemical system. But it farther appeared to me probable, that it operates likewise by its affinity to the acid, as there is reason I think to suspect, that the agency of water is important with regard to all acids. It is still however a case in point, since it is an example of the powerful agency of water in modifying affinities, and of a decomposition not being capable of being effected unless it be supplied; as well as a proof that a body, whether it be an earth or an acid, cannot be obtained insulated from its combinations, unless it receive the portion of water which it requires.

Some water retained in muriatic acid gas from its powerful affinity for it.

The argument, that from the strong affinity of muriatic acid to water a portion of it will be retained in combination with it in the elastic state, I hold valid, notwithstanding the reply, that muriatic acid does not exist as a gas in combination with different proportions of water, "for the proportion of hidrogen gas produced from muriatic acid gas, acted on by different metals, is always the same, whether it has been exposed to the influence of drying salts or not." The inference which would render this reply conclusive does not follow; admitting that muriatic acid gas cannot exist with different proportions of water, the only inference then would be, that muriatic acid gas obtained by the usual methods is in as dry a state as we can obtain it insulated. But I doubt altogether the alleged fact; for Dr. Henry found by

by the more unexceptionable mode of the action of electricity, that recent muriatic acid gas affords $\frac{1}{14}$ of its bulk of hidrogen, while after the full action of muriate of lime it affords only $\frac{1}{18}$ of its bulk. The inference in the reasoning on this subject, from the affinity of potassium to oxigen, arises from a misconception of the theory of Berthollet, on which the original argument is founded, so obvious that it does not require to be pointed out.

The author's experiments accurate.

Mr. Davy proceeds to some observations on my experiments, on which I shall offer few remarks, as I shall probably have an opportunity of engaging in the discussion of this part of the subject, to greater advantage, when better acquainted with the experiments brought forward in opposition to those I have stated. I may be allowed to say, that of the accuracy of the results I obtained, particularly of those which appear to be questioned, the formation of carbonic acid, when carbonic oxide, hidrogen, and oximuriatic acid gasses are submitted to mutual action, I am fully convinced. I had preserved the notes of these experiments, some of them written by Mr. Ellis, others by myself, at the time they were made; and in all of them carbonic acid was formed, though there often remained a sensible quantity of carbonic oxide, of which I have taken notice in my paper. How Mr. Davy infers, that the whole carbonic oxide should be converted into carbonic acid, in order to admit the inference, that oxigen is communicated from the oximuriatic acid, I do not comprehend. It is sufficient, if a portion of carbonic acid is produced. No notice perhaps is due to the remark, that in one of my experiments a small residue of common air was observed, whence it is inferred, that the conversion of carbonic oxide into carbonic acid might be owing to the presence of atmospheric air. Carbonic oxide gas requires for its conversion into carbonic acid by detonation with atmospheric air a quantity equal at least to $2\frac{1}{2}$ of its bulk; and not a twentieth of this quantity could have been introduced even in the most inaccurate mode, in which Mr. J. Davy may suppose the experiment to have been performed. The small portion observed had originated from the minute quantity disengaged by the force of the detonation from between the sides of the tube and the quicksilver,

silver, which, added to what might be disengaged by the transmission through the water, and agitation with it after the explosion, and to the slight admixture to which all elastic fluids are liable, (expressed in one of Mr. Davy's own experiments, by the phrase of "no more impurity than might be expected in the air in the gasses), had formed a quantity capable of being discovered. It could obviously detract nothing from the conclusiveness of the experiment, independent of the circumstance, that it was not observed in the more decisive experiment of the slow mutual action of these gasses.

He is willing to admit the accuracy of my experiments, so far as they relate to the want of action between carbonic oxide and oximuriatic acid, when water is excluded. This is a fact, which, though it presents an anomaly in the new hypothesis, is not absolutely hostile to it; and I have, it seems, "very satisfactorily proved" this nonaction, and that without the agency of water no carbonic or muriatic gas is formed. But my experiments, which afford results presenting a difficulty in the theory, are it seems inaccurate; and beside endeavouring to make this apparent by various observations, Mr. Davy informs us, that either alone, or in conjunction with his brother, he has, if not repeated them, (which would have been the most direct mode of proving their inaccuracy), at least made experiments of a similar kind, and with very different results. Thus he has performed some of the experiments of Cruickshank on the carburetted hidrogen gasses. The source of fallacy, which I supposed would have been contended for, in the presence of a portion of oxigen in the composition of these gasses, he seems to think of little importance; the principal source of fallacy is supposed to be in the presence of water, and he therefore made the detonations over mercury, and "never obtained carbonic acid gas, though oximuriatic gas in great excess was employed." I shall not say any thing of the source of fallacy, which I conceive has given rise to this observation, until I have repeated the experiment with the necessary precautions. I shall only observe, that Mr. Davy does not directly deny, that carbonic acid is formed, he has only performed the experiment without obtaining it, and he does not inform his

Mr. Davy's experiments.

readers what were the actual results. The hidrogen must of course have been removed by the action of the oximuriatic acid: What became of the carbon? Was it precipitated in the state of charcoal? Or did it remain in the state of carbonic oxide? Or did the gasses form a ternary combination? and how was the absence of carbonic acid established? Yet with all these deficiencies a result thus generally stated is brought forward in opposition to experiments minutely detailed. Neither does he deny the formation of carbonic acid in my experiment of submitting carbonic oxide, hidrogen and oximuriatic gasses to mutual action: he supposes its formation owing to the presence of atmospheric air, as I have already stated, or to the introduction of water after the experiment; the gratuitous assumption being made of the formation of a triple compound of carbonic oxide, hidrogen, and oximuriatic acid, by which this water would be decomposed. Hence, in the experiment made in conjunction with his brother, ammonia was used instead of water to absorb the muriatic acid, and it is inferred, that no carbonic acid was formed. It is singular however, that no attempt appears to have been made to discover this acid, its nonformation is inferred from the gas which remained after exposure to water burning with the same coloured flame as carbonic oxide, whence it is concluded to have been this gas, with the intermixture of nitrogen from the ammonia, its volume being admitted to be different from that of the carbonic oxide employed; and this negative result, open to such obvious fallacies, is placed in opposition to the positive production of carbonic acid in my experiments. Lastly, after all these experiments made without carbonic acid being obtained, and after the labour bestowed in endeavouring to prove experiments in which it was produced inaccurate, or to account otherwise for their results, a gas is discovered (the discovery of it is mentioned in a note, it having been made after Mr. J. Davy's paper was written) which it is admitted is capable of converting carbonic oxide into carbonic acid, and which is procured from the same materials as oximuriatic acid, and by a process apparently not much different from that which is usually followed.

Action of hidrogen, carbonic oxide, and

There remains one point to which it is necessary to give some consideration. Mr. Davy gives a reason why, as he supposes,

supposes, the presence of hidrogen cannot, even on the hypothesis I maintain, favour the conversion of carbonic oxide into carbonic acid by oximuriatic acid. In submitting these three gasses to mutual action, the hidrogen, in combining with oxigen from the oximuriatic, can form no more water than is required to the constitution of that portion of muriatic acid with which this oxigen had been combined, and hence there is no superfluous water to be afforded to that portion of muriatic acid, which remains to be formed in the oxigenation of the carbonic oxide by the remaining oximuriatic acid. "It is impossible therefore," says Mr. J. Davy (in the style which he permits himself to use) "that the latter portion of muriatic acid can become gaseous;..... yet it does become, according to the experiment, muriatic acid gas, which is a contradiction, and by itself a proof of the innacuracy of the hypothesis." oximuriatic acid.

This objection had occurred to me, and I had stated it as well as the solution of it to some of my friends. Of course however, a difficulty of this nature could not be urged against the fact, that the conversion of carbonic oxide into carbonic acid, which does not happen when oximuriatic gas alone is added to the former gas, takes place when there is an addition of hidrogen. And as farther the difficulty appeared to me capable of being solved, I thought it unnecessary to add to the length of my paper by taking notice of it. It is explained, I conceive, in the following manner. Muriatic acid gas may be formed with a less proportion of water than is necessary to its full saturation; the proof of its existence with an inferior proportion of water to what it contains at its first disengagement in the usual process for obtaining it I have already stated, and there is nothing improbable in the supposition, that it may exist gaseous with even less water than can be inferred from that proof. And in all elastic fluids small portions of water I believe exist, of which it is probably impossible to deprive them entirely. When a mixture, therefore, of carbonic oxide, hidrogen, and oximuratic acid gasses is submitted to mutual action, the hidrogen attracting oxigen from the oximuriatic acid, and forming with it a portion of water, this water may favour the production of a larger quantity of muriatic acid, than the quantity merely with which that oxigen had been combined, if Explanation of the difficulty.

any powerful affinity is at the same time exerted, having a tendency to produce that acid: now such an affinity is actually exerted by the carbonic oxide, hence it is enabled to operate with effect, an additional portion of oximuriatic acid is decomposed, and by the communication of oxigen portions of muriatic and carbonic acids are formed. This is further aided by the portion of water which must have been contained in the portions of hidrogen, carbonic oxide, and oximuriatic gasses, suffering these changes, even though they had been previously rendered as dry as possible. Of itself, this would not be sufficient to favour the decomposition of the oximuriatic acid, but added to the quantity formed by the oxigenation of the hidrogen it may be adequate to this effect, to the extent I have stated. The operation of this small quantity of water, remaining in elastic fluids in the driest state to which they can be brought, is very well shown in the results of the detonation of hidrogen and oximuriatic gasses, as related by Mr. H. Davy. Mr. J. Davy, indeed tells us, that he has seen this experiment made a number of times, "and though correctly made, there is no condensation attending their union." But his brother informs us, that, when the gasses are mixed over water, introduced into an exhausted vessel, and fired by the electric spark, there is a condensation of from $\frac{1}{10}$ to $\frac{1}{15}$ of the volume. I have attempted, he continues, "to make the experiment in a manner still more refined, by drying the oximuriatic acid, and the hidrogen, by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures, but I have never been able to avoid a slight condensation, though in proportion as the gasses were free from oxigen or water, this condensation diminished." This condensation must be owing to the water remaining in the gases even after the action of muriate of lime; and it is this, no doubt, which partly operates in the experiments I have related, and favours the result.

Conversion of carbonic oxide into acid by oximuriatic gas with a little water.

Without any reference to this experiment I would further observe, that the conversion of carbonic oxide into carbonic acid, when oximuriatic acid acts upon it with the admission of a little water, is conclusive against Mr. Davy's hypothesis. The only mode in which the result can be accounted

counted for, in conformity to that hypothesis, is to suppose the water to be decomposed. This I had anticipated, and had observed, that of this supposition we have not only no proof, but we have sufficient reason from known facts to reject it. "Water is not decomposed by oximuriatic acid gas, or by carbonic oxide gas; there is no reason to conclude, that its decomposition can be effected by their action, when they are presented to it merely in a mixed state; and the more obvious operation may be regarded as the real one, that it acts by its affinity to muriatic acid."

In answer to this, Mr. J. Davy remarks, have we not the most indubitable evidences of the decomposition of water by oximuriatic gas? "Pass this gas and steam together through a tube heated to redness, oxigen gas will be produced, and muriatic acid gas formed. Detonate this gas and hidrogen together, and a similar formation of muriatic gas will take place. With these facts in view, who can hesitate in asserting, that water is decomposed by oximuriatic gas, that its hidrogen is attracted by this substance, and its oxigen consequently set free."

Detonation of oximuriatic gas with hidrogen.

The decomposition of oximuriatic gas by detonation with hidrogen gas, or its combination with that gas, according to Mr. Davy's view, affords no proof of the power of water to decompose it. Admitting the decomposition of oximuriatic gas by water at ignition, and supposing that we know nothing as to the mutual action of these substances at low temperatures, the conclusion would not follow, that at such temperatures this decomposition would take place. But to draw this conclusion in the very face of the fact itself is, I confess, a mode of reasoning altogether new to me. We know, that oximuriatic acid is not decomposed by water at natural temperatures; and knowing this I have no hesitation in assserting, notwithstanding Mr. Davy's indubitable evidences, that the supposition of water being decomposed in the slow mutual action of carbonic oxide and oximuriatic gasses is altogether gratuitous, and made to avoid a difficulty, which cannot otherwise be explained.

Conclusion.

And lastly let it be remarked, that putting aside all these experiments the two hypotheses still remain at least on equal grounds. The experiments were supposed to afford

afford evidence in favour of the common doctrine of the nature of the relation between muriatic and oximuriatic acids, and were they proved to be inconclusive, it would only stand in the same rank with the opinion advanced by Mr. Davy; it can be applied, as I trust I have shown in my former paper, to the explanation of all the phenomena, and to some of them with more advantage than the other; and it has besides, as an hypothesis, that superiority from general considerations, which I have also stated to belong to it.

I am, with much respect,

Your most obedient servant,

JOHN MURRAY.

IX.

On the Production of Hyperoximuriate of Potash, considered with respect to Mr. DAVY'S *Ideas of the Nature of Oximuriatic Acid. In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

Mr. Davy's theory of oximuriatic acid applied to the hyperoximuriate of potash.

I Beg leave to trouble you with a few observations concerning the new theory of the oximuriatic acid as advanced by Mr. Davy, more especially as to its application to the phenomena attending the production of the so called hyperoximuriate of potash. If you think them worthy of a place in your valuable Journal, I shall consider myself honoured by their insertion. To Mr, Davy I shall offer no apology for the freedom, with which I have treated the subject; as I am convinced, that that distinguished gentlemen is more anxious for the establishment of truth, than the introduction of a favourite hypothesis.

Peculiarity of his theory.

The peculiarity of Mr. Davy's theory consists in considering the oximuriatic acid as a simple substance, a peculiar acidifying principle of a nature something analogous to oxigen. Muriatic acid he supposes to be a combination of this principle with hidrogen. He appears to me not quite to

to have made up his mind, whether this new compound enters, like other acids, into union with the different bases, or whether in the act of such union the hidrogen is again abstracted: for in one place he states, "that the compounds, "which have been usually considered as *muriates*, or as "dry muriates, are properly combinations of *oximuriatic* "acid with inflammable *bases**," thus setting aside any union with the muriatic acid: and in another he asserts, that "muriate of ammonia is a compound of *muriatic* acid "*gas* with ammonia"†.

The process for forming the hyperoximuriate of potash, to which I purpose to confine my observations, is this.— Black oxide of manganese is put into a retort connected with a Woulfe's apparatus, into the bottles of which a solution of potash is poured. Muriatic acid is added to the manganese, and heat applied. Formation of hyperoximuriate of potash.

Oximuriatic acid passes over and combines with the potash; and after the operation is completed, *two* distinct salts are found in the bottles, viz. *hyperoximuriate*, and common *muriate* of potash.

The muriatic acid is of course the body, from which the oximuriatic is to be derived, by some change operated in it by the oxide. The new theory says, that muriatic acid is a combination of oximuriatic with hidrogen. The oxigen then of the manganese combines, from superior affinity, with this hidrogen to form water, and thus liberates the oximuriatic acid. So far the application might be deemed satisfactory. The new theory accounts for the production of oximuriatic acid:

Let us now proceed to account on the same principles for the phenomena which occur, when this peculiar acidifying principle comes into contact with the solution of potash. There are two ways in which this may be attempted.—We must suppose, as Mr. Davy seems inclined to do, that *muriate* of potash is a combination of *oximuriatic* acid with *metallic potassium*, or that it is a compound of *muriatic* acid with *potash*. but how does it apply to the formation of the hyperoximuriate of potash?

Let it be granted, that it is a combination of oximuriatic acid and potassium. When the oximuriatic acid (the form- Difficulties in the 1st. supposition.

* Phil. Trans. 1810: or Journal, vol. XXVII, p. 327.

† Ibid. p. 328.

ation

ation of which we have accounted for) comes into contact with the solution of *oxide* of *potassium*, we must suppose, that part of it from superior affinity displaces *part* of the oxigen, and combines with the potassium. But how shall we in the first place account for this partial action? If a superior affinity exist between *part* of the oximuriatic acid and *part* of the potassium, how is it that it does not subsist between the whole? How is it, that the whole oxigen of the potash is not set free, and the combination consist of muriate of potash only?

But what becomes of that portion of oxigen, which is thus liberated? Does it unite with the remainder of the oximuriatic acid? and so united, do they combine with the remaining oxide of potassium? Or is it attracted by the already saturated *oxide*, and that too in the face of a superior affinity?—This is establishing a new law of affinity at once. We assert, that *part* of the oximuriatic acid will not combine with the oxide of potassium, but overcomes a powerful* affinity to form a separate combination; and, that another part not only does unite with the same oxide, but solidifies along with it that portion of oxigen, with which the first part was incapable of uniting. To anomalies, or, more correctly speaking, to contradictions like these, it would be difficult to bring the mind to assent, even if there were no other means of accounting for the changes in the process.

Difficulties on the 2d.

Let us now examine the second supposition, under which this new theory may be attempted to be applied, viz. that muriate of potash is a compound of muriatic acid and potash. We must now suppose, that, when the oximuriatic acid first enters the solution of potash, part of it abstracts from the water of the solution a portion of hidrogen; and, being thus changed to muriatic acid, combines with the potash to form muriate of potash. The oxigen thus liberated unites to the other portion of the oximuriatic acid, and forms with the remainder of the potash the hyperoximuriate.

Now, setting aside the anomaly of a simple substance being capable of uniting either with oxigen or hidrogen to

* Mr. Davy states it to be an intense affinity. See as above, p. 330.

form

form with each of them an acid compound, (a difficulty of which Mr. Davy is aware) observe to what an insurmountable inconsistency this leads. We first of all account for the decomposition of the muriatic acid in the retort by an affinity between oxigen and hidrogen superior to that between hidrogen and oximuriatic acid, and we now account for the formation of the salts by an affinity between hidrogin and oximuriatic acid superior to that between hidrogen and oxigen. This is a complete contradiction in terms, and of course the hypothesis is untenable for a moment.

This inconsistency, permit me to remark, appears upon the face of Mr. Davy's own memoirs. He ascribes the production of water, when muriatic acid gas is passed over litharge, to the superior affinity between *oximuriatic* acid and *lead*, and the consequent union of the hidrogen of the one and the oxigen of the other *; and he accounts for the oxide of tin and the muriate of ammonia, obtained by ammonia, upon adding water to Libavius's liquor, by a superior affinity between *oximuriatic* acid and *hidrogen* †. In the first place water is *composed* because the affinity of oximuriatic acid for a *metal* is *greater* than the quiescent affinities, taken together, of oximuriatic acid for hidrogen and the metal for oxigen; and in the second, water is *decomposed* because the affinity of *oximuriatic* acid for a *metal* is *less* than the now divellent affinities of oximuriatic acid for hidrogen and the metal for oxigen. Farther anomaly.

Now unless it can be shown, that the oximuriatic acid has an affinity for lead *very far indeed* superior to what it has for tin (which cannot be done, for the difference is on the other side, and thus adding to the difficulty) it is plain, that this is as complete a contradiction as that which we have just noticed.

I should wish to enter more fully into this subject, but I have already exceeded the limits which I had prescribed to myself. At some future time I may resume the discussion.

I trust, that what I have said may have this benefit, that it will lead to farther investigation, and thus prevent the too hasty adoption of an hypothesis, not yet sufficiently grounded upon facts and experiments, and the too incon-

* See as above, p. 326. † Ib. p. 327.

siderate

siderate rejection of a theory, beautiful on account of its unity and simplicity.

I am, sir,

Your most obedient servant, and constant reader,

F. D.

19*th March*, 1811.

X.

Observations respecting the Irritability of the Barberry, Berberis communis. By Mr. ROBERT LYALL, *Surgeon, Member of the Royal Physical Society of Edinburgh, and corresponding Member of the Literary and Philosophical Society at Manchester. Communicated by the Author.*

Irritability of the stamens of the barberry.

ALTHOUGH it was known many years ago, that the *stamina* of the barberry contracted when irritated by mechanical stimuli, yet till the time when Dr. Smith published his observations relative to the subject *, our knowledge on this point was but incomplete.

Dr. Smith's experiments on them.

Dr. Smith, in order to ascertain what part of the stamina was endued with irritability, cut off one of the petals of the flower so carefully as not to touch the stamen, that stood next it; then with a very slender piece of quill he touched the outside of the filament, which had been next the petal, but it remained perfectly immovable. With the same instrument he then touched the back of the anthera, then its top, its edges, and at last its inside; still without any effect.—But the quill being carried from the anthera down the inside of the filament, it no sooner touched that part, than the stamen sprung forwards with great vigour to the stigma. From the result of this and other experiments Dr. Smith concludes, "That the motion above described was owing to a high degree of irritability in the inside of each filament, next the germen, by which, when touched, that side becomes shorter than the other, and consequently the filament is bent towards the germen."

Manner in which he accounts for it.

* Phil. Trans. for 1786.

Bonnet's

Bonnet's opinion is quite in opposition to Dr. Smith's; for he thinks, that the movement of the stamina of the barberry so much resembles the motion of a spring which unbends itself, that it cannot be attributed to irritability: an opinion certainly that requires only the evidence of seeing the experiment performed, to refute it entirely. Would even the most elastic spring, formed like the stamina of the barberry, and like them also simply fixed by one end (as if one end of the spring were nailed to a board, and not fixed by any other mechanical contrivance) move when merely touched gently at a particular part of the fixed end? certainly not. It may be observed indeed, that, if the loose end of the spring were touched gently, a vibratory motion would be produced; but this motion is very different from the gradual and steady movement of the stamina of the barberry.—Why then employ such an hypothesis, to account for their motions? Bonnet's opinion.

Sennebier, when treating of the barberry, speaks thus: "It has not yet been proved, that the movement of the stamina is accompanied with the contraction of the filaments, which nevertheless was the first proof necessary to have been given, in order to ascertain their irritability; it is not yet even well known, which is the irritable part of the filaments, and whether it be only their base, as Smith has had the address to discover." How Sennebier came to express himself in this ambiguous manner is to me a matter of surprise.—Indeed the different parts of this sentence seem to contradict each other. Sennebier's remarks on it.

Dr. Smith, as we have seen, has stated in a very perspicuous and definite manner, that the inside of the base of the filament possesses a high degree of irritability; and his experiment, which I have quoted, shows evidently, that this part only is irritable. He has said also, that one side of the filament becomes shorter than the other. Why then is it stated by Sennebier, that we even do not well know which is the irritable part of the filaments, and whether it be only their base, *as Smith has had the address to discover?* Why does Sennebier confess, that Smith has had the address to discover, that the base of the filament only is irritable, immediately after he has asserted, that we are ignorant

rant regarding the irritable part of the filament? Certainly, Sennebier did not doubt the accuracy of Dr. Smith's observations; and had he repeated the Doctor's experiments before he wrote, I think, he would never have treated of the irritability of barberry in the manner which he has done.

Dr. Smith's experiments repeated,

Some of the above remarks were written last year, at a period, when it was not in my power to repeat Dr. Smith's experiments. This season however presenting ample opportunity for the purpose, I readily embraced it; and now it is with pleasure I state, that, on comparing my remarks with those of Dr. Smith, they perfectly coincided: and that I have no hesitation in saying, that the inside of the base of the filament is the *only* irritable part of the stamina.

and varied.

To ascertain the last mentioned point clearly, and that I might be allowed to experiment easily, I cut or pulled off the calyx and corolla from a number of flowers: and, not being careful to avoid irritation, the contraction of the stamina always took place.—In a few minutes after, however, the stamina completely expanded: and then I repeated Dr. Smith's experiments, as well as others, sometimes without detaching the flowers from the bush, at other times with flowers which I had carried to my room.

The motion of the stamens takes place under water.

The motions of the stamina, I observed, were performed with as much vigour under water, as when the flowers were growing on the bush.

They will contract a 2d time in a few minutes.

I likewise ascertained, that the stamina, after being once contracted, were capable, in healthy flowers, of undergoing a second contraction, after an interval of five minutes, or sometimes in a much shorter period.

At a future time, I intend publishing the result of my experiments on the barberry with chemical stimuli.

X.

XI.

Extract of a Letter from the Rev. JOHN BRINKLEY, *D. D. F. R. S. Andrews Professor of Astronomy in the University of Dublin, to the Rev.* NEVIL MASKELYNE, *D.D. F. R. S. Astronomer Royal, on the annual Parallax of α Lyræ.*

I HAVE now had sufficient experience of my eight feet circle, to be highly satisfied with it, and have arrived at one conclusion, that it is of importance in astronomy.

Annual parallax of α Lyræ.

My observations on α Lyræ for the purpose of discovering an annual parallax now amount to 47 in number, viz. 22 near opposition, and 25 near conjunction, and the mean of these gives a result of 2·52″ as the parallax of the annual orbit for that star; and I have no doubt, that it exceeds 2″.

Dr. Bradley's formula for refraction

My observations of different circumpolar stars, and of the same star in different states of the thermometer, seem to require a small alteration in the numbers of Dr. Bradley's formula for refraction.

altered

The formula so altered is—Refraction $= 56{\cdot}9'' \times \text{tang.}\left\{\text{Zen. dis.} - 3{\cdot}2\ \text{Refr.}\right\} \times \frac{\text{height of barom.}}{29{\cdot}6} \times \frac{500}{450 + \text{ther.}}$

By means of this formula, the observations of circumpolar stars considerably distant give the same colatitude to a great degree of exactness.

SCIENTIFIC NEWS.

Wernerian society.

AT the meeting of the Wernerian Natural History Society, at Edinburgh, on the 12th of January last, Professor Jameson read the first part of a series of observations on the geognostic relations of the rocks in the island of Arran. In this memoir he described particularly the granite, gneiss, mica-slate, and clay-slate formations, and also the red sandstone and porphyry-slate, which occur so abundantly in that island. When describing the granite, he stated, as a conjecture

Geognosy of Arran.

Quartz perhaps older than granite.

jecture, that quartz might prove to be an older formation than granite, because the oldest granite contains much quartz, but little mica, and less feldspar than the newer varieties. He pointed out several observations to be made, with the view of verifying or of refuting this conjecture. In his description of gneiss, he alluded to the veins of granitic gneiss which traverse it, and which, when the gneiss and granite are in contact, have been represented as veins of granite shooting from the subjacent into the superincumbent rocks. The red sandstone the professor appeared inclined to refer to the first or old red sandstone of Werner. When describing its stratification and structure, he pointed out the appearances that ought to be attended to

Direction and dip of strata.

in endeavouring to ascertain the dip and direction of strata, and particularly cautioned observers against confounding the structure of individual strata or beds with the direction and dip of the strata. The numerous fissures that traverse the sandstone of Arran, and which exhibit every variety of magnitude, direction and dip, afforded apt illustrations of Werner's theory of veins. The porphyry-slate the Professor described as appearing in the form of overlying conical, or irregular tabular-shaped masses, resting on the red sandstone, also in veins traversing granite, sandstone, greenstone, and other rocks. He gave a description of some tabular masses of this rock, accompanied with pitchstone and claystone, contained between strata of sandstone, and which might be confounded with beds; but which he was inclined to consider merely as lateral branches of veins, or as slightly inclined veins.

Geognosy of Arran continued.

At the next meeting, on the 2d of February, Professor Jameson read the continuation of his mineralogical observations on Arran. He first detailed the geognostic relations of the floetz greenstone of that island. From this account it appeared to occur in overlying masses resting on sandstone, in beds in sandstone, and in veins that traverse sandstone and other rocks. He next described the various kinds of pitchstone that occur in Arran, and seemed to think one of the varieties was so well marked, that it might constitute a distinct subspecies of the pitchstone. The account of its geognostic relations afforded a detail of many curious geognostic

geognostic appearances, in particular the structure of its veins, and the nature of the interposed tabular masses, having many of the characters of beds, yet appearing to be either nearly horizontal veins, or lateral branches of common veins. The claystone of Arran, which was next described, appeared to occur in overlying masses along with the porphyry-slate, and also in veins along with pitchstone and porphyry-slate. It would seem that wacke and basalt are not very frequent or abundant rocks in Arran; but when they are observed, they present the usual appearances and geognostic relations.

From the observations in these two memoirs it appears, that this island contains no transition rocks; but is principally composed of primitive and floetz rocks. The alluvial rocks that occur in the valleys present the usual characters of the rocks of this class.

Mr. Singer informs me, that he has prepared some novel apparatus for the application of common electrical powers to the purpose of chemical analysis, for the illustration of his lectures on the chemical agencies of electricity, the first of which will be given on thursday, the 11th of april. His galvanic battery exposes upward of 14000 square inches of zinc surface.

TO CORRESPONDENTS.

Mr. Gowar's paper was too late for insertion in the present number.

ERRATUM.

Page 197, *line* 22, *for* hypothesis; for the, *read* hypothesis. Farther,

METEOROLOGICAL

METEOROLOGICAL JOURNAL,

For MARCH, 1811,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

Day of FEB.	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
27	44°	44°	48	40°	29·31		Clo udy	Fair
28	47	47·5	51	40	·42	·030	Rain	Cloudy
MAR.								
1	43	42	46	38	·61	·075	Fair	Fair
2	47	48	51·5	46	·82		Ditto	Fair†
3	48·5	47	53·5	45	·80	·010	Ditto	Ditto
4	49	49	54	46	30·02		Ditto	Ditto
5	47	50	51·5	43	29·74	·005	Ditto	Rain‡
6	44	50	51	48·5	·48	·080	Ditto	Ditto
7	51	49	52·5	47	·38	·215	Rain	Rain
8	49	43	49	36	·35	·195	Ditto	Cloudy
9	38	40	43·5	37	30·26	·110	Fair	Fair
10	42	46	50	42·5	·47		Ditto	Ditto
11	44	47	50	44·5	·46		Ditto	Foggy
12	47	47	52	40	·43		Ditto	Cloudy
13	44	45	51	41	·38		Ditto	Ditto
14	44	44	47	38	·34		Ditto	Ditto
15	41	42	48	36·5	·35		Ditto	Fair
16	40·5	44	49·5	37	.29		Ditto	Ditto
17	41	44	50	37	·16		Ditto*	Ditto*
18	40·5	45	55	42·5	·14		Ditto*	Ditto
19	48	47	55	43·5	.18		Ditto	Ditto
20	49	50·5	53	49	.15		Cloudy	Cloudy§
21	51	52·5	56·5	47	.03	·040	Ditto	Rain
22	49	44	49	37	·15	·020	Fair	Fair
23	42	45	52	38	·50		Ditto	Ditto
24	46	45·5	54	40	·37		Ditto	Ditto
25	46	44	52	39	·18		Ditto	Ditto
26	46	45·5	55	38	·07		Ditto	Ditto
27	43	45	54	36	·30		Ditto	Ditto
28	39	46	51·5	40	·47		Ditto	Ditto

·780 Inch. since last Journ.

* Intervening Fogs. † Cloudy and boisterous after 11. § Rain in the Night.

‡ The early part of the Evening remarkably fine, giving me an opportunity of observing Jupiter, with a four-feet reflector, with a power of 400.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXVIII.

ARTICLE I.

On the Gaseous Combinations of Hidrogen and Carbon. By THOMAS THOMSON, *M. D. F. R. S. E.* *

THE gasses formerly distinguished by the name of *heavy inflammable airs*, which are evolved during the distillation of most animal and vegetable substances, differ so much from each other in their properties, that it has been hitherto impossible to reduce them under a small number of species. When burnt, they all yield carbonic acid and water, and of course contain carbon and hidrogen; but the proportion of these bodies formed, and of oxigen consumed during the combustion, varies very much at different times. Many of these gasses have been examined with much address by Cruickshank, Dalton, and Henry. Berthollet also has examined them at different times, and published very ingenious dissertations on their composition. Gaseous compounds of hidrogen and carbon.

From a great many experiments on these gasses, at different times, and in different states, I have convinced myself, that they usually hold an empyreumatic oil in solution; and that the differences in their specific gravity, and other properties, depend very much upon the proportion of oil Their differences depend much on the presence of oil,

* From the Wernerian Transactions.

present. Hence no pure gasses, fit for examination, and comparable with each other, can be obtained from those vegetable or animal substances which yield an oil when subjected to heat, as is the case with most of them. To this oil is to be ascribed the great variation in the specific gravity of the gas obtained by distilling pit-coal, as shown by the experiments of Dr. Henry.

and carbonic oxide.

When water and carbon are present together, as is the case in most animal and vegetable substances, they act on each other, and give origin to variable quantities of carbonic oxide, which must also very much alter the properties of the gas evolved. To these two causes, namely, the oil dissolved, and the carbonic oxide formed, most, if not all the varieties in the combustible gasses, obtained from animal and vegetable substances, are owing.

Supposed to contain oxigen.

Berthollet, in a dissertation which he lately published, in the second volume of the Mémoires d'Arcueil, has endeavoured to prove, that all the heavy inflammable gasses contain oxigen as a constituent. He has examined a number of gasses obtained by distilling charcoal, and has shown, that each of them contained a considerable proportion of oxigen. This opinion has likewise been maintained by Mr. Murray in his System of Chemistry.

If any confidence can be put in the preceding observations, it is clear, that this obscure subject can never be elucidated by examining gasses from charcoal, or from animal and vegetable substances. The first will always yield carbonic oxide as well as carburetted hidrogen, and the gas from the other bodies will be disguised by the oil held by it in solution.

A pure carburetted hidrogen probably exis s.

Analogy is strongly in favour of the common opinion, that there exists a gas composed simply of hidrogen and carbon. Hidrogen, we know, is capable of dissolving every other simple combustible, sulphur, phosphorus, and boracium. It dissolves likewise several of the metals, as arsenic, zinc, tellurium, and potassium. Why then may it not be capable of dissolving carbon?

Gas from stagnant water examined.

There is a gas which rises in considerable quantity from stagnant waters during the summer season. It was examined by Cruickshank and Dalton, and both of them concluded

cluded it to be a compound of carbon and hidrogen, without any sensible quantity of oxigen. But as neither of these gentlemen has published a detailed account of his experiments, I thought it worth while to examine the gas anew, in order, if possible, to determine the point in a satisfactory manner. I accordingly collected considerable quantities of it at different times in the neighbourhood of Restalrig, where ponds of muddy water are left stagnant, in order to collect manure. This gas I found to have the following properties.

Its properties.

1. It is colourless, and transparent like air.

2. It has no smell, and no sensible taste, provided it be previously washed in clean water.

3. It always contained a mixture of carbonic acid. The least quantity of this gas present was 5 per cent; the greatest 7½ per cent.

4. It always contained a mixture of common air. It is remarkable, that the proportion present was in every case the same, and amounted to 12·5 per cent, or 2·5 per cent of oxigen and 10 per cent of azote. Mr. Dalton says, that the gas which he examined contained 20 per cent of azote. This was never the case with the gas from Restalrig; but the common air and carbonic acid gas together sometimes amounted to 20 per cent, and always to near that quantity.

5. After depriving it of its carbonic acid, I found its specific gravity 0·611, that of air being 1·000. But as it contained a mixture of 12·5 per cent of air, it would have been obviously lighter, if this portion had been removed. By calculation I find, that the specific gravity of the pure gas would have been 0·5554*.

Method of taking the specific gravity of gasses.

My method of taking the specific gravity of gasses being extremely simple, yet precise, I think it worth while to describe it in this place. It is founded on the well-known fact, that, when two gasses are mixed, their bulk does not

* Let x = the specific gravity of the pure gas, A = the quantity of air in the mixture, a = the specific gravity of air, B = quantity of pure gas present, c = specific gravity of the mixture, we have $x = \frac{(A+B)c - Aa}{B}$.

alter. I have a large flask fitted with a stop-cock. I weigh this flask very accurately, then exhaust it, and weigh it again. Let the loss of weight be a. I then fill the flask with the gas, the specific gravity of which I want, and weigh it again. If the weight of the flask be the same as at first, it is obvious, that the gas has exactly the specific gravity of common air. If it be heavier than at first, the gas is heavier than common air; if lighter, the gas is lighter than common air. Suppose the flask lighter than at first, I add weights till the flask is exactly poised. Let the weight added be b. We have the weight of common air to that of the gas as a to $a-b$. And to find the specific gravity, we have this proportion, $a : a-b :: 1 : x =$ specific gravity wanted, or $x = \frac{a-b}{a}$. If the flask is heavier than at first, weights are added to the other scale, till it is exactly poised; let these weights, as before, be b, we have in this case $x = \frac{a+b}{a}$. In this process no correction is necessary for temperature, or the state of the barometer; because air and all gasses undergo the same change of bulk, by changing the temperature and the pressure. The gas is always weighed saturated with moisture. This is corrected when requisite by the table of Dalton. In this way the flask may be easily weighed, true to the $\frac{1}{100}$dth part of a grain; so that you are always sure of the specific gravity of the gas, to the 3d decimal figure. The specific gravity of common air is always taken 1·000, and at the temperature of 60°, barometer at 30 inches, 100 cubic inches of it weigh 30·5 grains. These data enable us to determine the weight of 100 cubic inches of any gas the specific gravity of which is known. Mr. Dalton found the specific gravity of this gas 0·600. I took its specific gravity more than once, and collected at different times, but never found it heavier than 0·5554.

Farther properties of the gas from stagnant water.

6. The gas from stagnant water burns with a yellow flame, more readily, and with a larger flame, than any other combustible gas, except olefiant gas, and the vapour of ether.

7. When mixed with oxigen gas, and kindled by an electric spark, it detonates loudly, and undergoes a considerable

ble diminution of bulk. Its combustibility by this process is confined within very narrow limits. It does not burn, unless the bulk of the oxigen rather exceeds its own bulk, and it ceases to burn, when the oxigen gas is more than $2\frac{1}{4}$ times the bulk of the inflammable gas. The limits of combustion, according to my trials, are 100 measures of inflammable gas, and 105 or 227 measures of oxigen gas. A mixture of 100 measures of inflammable gas with 282 or 292 measures of oxigen gas does not burn.

8. A good many experiments on the combustion of this gas with oxigen by electricity, give the following general result: 100 measures of the inflammable gas consume 205 measures of oxigen gas, and 104 measures of carbonic acid gas are formed. This result very nearly agrees with the experiments of Mr. Dalton, who found, as I have been informed by Dr. Henry, that 100 measures of this gas require for combustion 200 measures of oxigen gas, and form 100 measures of carbonic acid. The following table contains a few of the experiments which I made upon this gas. The others were precisely of the same kind, and coincided with these as nearly as possible. Experiments made on this gas.

The oxigen gas used contained 12 per cent of azote. It had been prepared from hyperoximuriate of potash, and, when recent, contained only 1 per cent of azote. It had been kept above a year in a crystal bottle well stopped; but had been often opened, and portions of it used during the interval. The bottle was now half full of water. The purity of this gas was tried just at the time of making the following experiments, by exposing it to hydroguretted sulphuret of lime in the usual way. The nitrous gas employed contained, by the test of sulphate of iron, 11·5 per cent of azote. The inflammable gas had been freed from carbonic acid, but it contained 13·5 per cent of common air.

Experiments.

	Measures of inflammable gas.	Measures of oxigen.	Bulk of residue.	Bulk of do when washed in lime-water.	Measures of nitrous gas added to do.	Bulk of residue.	Bulk of do. washed in sulphate of iron.
1	25	50	31	9	22	24	9
2	25	60	40	17	40	30	12
3	20	55	Does not burn.				
4	25	55	44	38	88	42	18
5	25	50	31	8·5	39	32	10
6	40	35	Does not burn.				
7	40	40	Burns.				

From this table, and our knowledge of the constituents of the gasses used, it is easy to deduce the following:

	Gasses examined.				Carbonic acid formed.	Residue.	Composition of Residue.	
	Pure inflammable gas.	Pure oxigen.	Azote.	Total.			Azote.	Oxigen.
1	21·6	44·7	8·7	75	22	9	8·7	0·3
2	21·6	53·5	9·9	85	23	17	9·9	7·1
3	17·3	48·9	8·8	75				
4	2·16	49·1	9·3	80	6	38	9·3	29
5	21·6	44·7	8·7	75	22·5	8·5	8·7	0
6	34·6	31·9	8·5	75				
7	34·6	36·3	9·1	80				

Of these experiments, the 1st, 2d, and 5th are the only ones, from which the proportion of oxigen consumed, and of carbonic acid formed, can be deduced. They were repeated

repeated very often, in order to insure as much accuracy as possible. The following is the result which they give:

Results.

Inflammable gas consumed.	Oxigen gas continued.	Carbonic acid gas formed.	
21·6	44·7	22	
21·6	43·6	23	
21·6	44·7	22·5	
21·6	44·3	22·5	average.
100	205	104	aver. per cent.

9. From these experiments it is easy to deduce the composition of this inflammable gas. Its specific gravity being 0·555, 100 cubic inches of it, at the temperature of 60°, and when the barometer stands at 30 inches, will weigh 16·93 grains. Its composition.

Of the 205 cubic inches of oxigen gas consumed, 104 go to the formation of carbonic acid gas; the remaining 101 cubic inches must have combined with hidrogen during the combustion, and formed water. Hidrogen, in the gaseous state, combines with exactly half its bulk of oxigen gas; therefore the hidrogen, which went to form water in the present case, must have been equivalent to 202 cubic inches.

If we suppose with Saussure, (and it is the supposition least favourable to our present purpose), that carbonic acid gas contains 27·5 per cent of carbon, in this case the carbon contained in 104 cubic inches of it must weigh 13·24 grains. The weight of 202 cubic inches of hidrogen gas is 5·17 grains. Thus we have ascertained, that 100 cubic inches of the inflammable gas from stagnant water contain of

Carbon,	13·24
Hidrogen,	5·17
Total,	18·41

But

But the weight of 100 cubic inches of the gas is only - - - - - 16·93

So that the constituents found by analysis exceed the weight of the gas - - 1·48 or almost 1.5 grain.

This is a clear proof, that the gas contains no oxigen. The carbon and hidrogen, which we have found it to contain, constitute the whole of its weight. The small excess would nearly vanish, if we were to suppose the specific gravity of the gas to be 0·600, as Dalton found it. It is obviously owing to errours in the analysis, which are unavoidable when minute quantities of gaseous bodies are measured.

10. We may conclude, then, that the gas from stagnant water is entitled to the name of carburetted hidrogen, and that it is a compound of very nearly,

Carbon - - -	72
Hidrogen - -	28
	100

Mr. Dalton considers it as composed of an atom of carbon, and two atoms of hidrogen; and this very nearly agrees with the above analysis, if we suppose an atom of carbon to weigh 4.5, and an atom of hidrogen to weigh 1; for 72 is to 28 very nearly as 4.5 to 2.

Olefiant gas. There is another gas, which is at present considered as composed of carbon and hidrogen. It was discovered by the associated Dutch chemists, and called by them Olefiant Gas, in consequence of the oily-looking substance into which it is converted, when mixed with oximuriatic acid gas. The experiments of Cruickshank, Henry, Dalton, and Berthollet, all coincide to show, that its only constituents are carbon and hidrogen. Mr. Murray, however, in his System of Chemistry, has expressed his suspicion, that oxigen is also one of its constituents. He founds his conjecture on the alleged formation of carbonic acid gas when olefiant gas is passed through a red hot tube. Even if carbonic acid were formed in this case, it would not prove the gas to contain oxigen, unless the experiment be made in such a manner, as that all common air is completely excluded;

cluded; and practical chemists are sufficiently aware of the difficulty of such an exclusion, when the experiment is made in the usual way. Carbonic acid gas never makes its appearance, if the whole common air be previously removed from the tube by means of a current of hidrogen, and if care be taken to separate all the oxigen, with which the olefiant gas may be mixed, by nitrous gas. It is proper to know, that liquid sulphuret of lime has the property of dissolving olefiant gas in considerable quantity. It cannot, therefore, be used to free the gas from oxigen.

Dissolved by sulphuret of lime.

Though the well-earned celebrity of the chemists, who have analysed olefiant gas, left little doubt, that their analysis was accurate, I thought it worth while to repeat their experiments, in order to add my testimony to theirs.

1. One ounce measure of alcohol, of the specific gravity 0·826, and 3 ounce measures of sulphuric acid, of the specific gravity 1·860, were mixed together, and boiled in a small retort, almost to dryness; while the gaseous products were received in the usual manner over water. The gasses obtained, (not reckoning the common air of the retort, which amounted to about 42 cubic inches), were 146·12 cubic inches of olefiant gas, and 53 cubic inches of carbonic acid gas. The proportion of carbonic acid at first was very small, but it increased asthe process went on, and at the end amounted to about one half of the olefiant gas. The charry matter which remained in the retort, after being well washed and dried on a sand-bath, weighed 68 grains.

Olefiant gas formed.

2. A portion of this olefiant gas, deprived of its carbonic acid, but containing 16 per cent of common air, was found of the specific gravity 0·9786, that of air being 1·000. Hence the specific gravity of pure olefiant gas is 0·9745. This is heavier than it was found by the Dutch chemists, who state its specific gravity at 0·909. As far as I know the specific gravity has not been taken before in this country.

Its specific gravity.

3. Being curious to know the nature of the supposed oil, which is formed when olefiant gas is mixed with oximuriatic acid gas, I filled a large bottle with olefiant gas, and passed a current of oximuriatic acid gas into it; the wished for substance soon collected at the bottom of the phial. It possessed the following properties.

Mixed with oximuriatic acid gas.

(1.) It

Properties of the substance formed.

(1.) It was a liquid of a greenish white colour, with a slight smell of oximuriatic acid, which it lost when allowed to stand a sufficient time exposed to the air. Its taste was sweet and cooling, and it made a strong, though not unpleasant, impression on the palate.

(2.) When dropped into water, it fell to the bottom, and looked like so much melted phosphorus. It continued in this state for some time if left at rest; but when the mixture was agitated, it dissolved in the water. The liquid continued colourless, acquired a sweet and cooling agreeable taste, and a slight aromatic odour. It did not affect vegetable blues, but precipitated copiously with nitrate of silver.

(3.) When dropped into alcohol, the liquid appeared milky at first, but almost immediately became transparent, a complete solution being effected.

(4.) Sulphuric ether dissolved it with great rapidity, and the solution was colourless.

(5.) It did not dissolve in oil of turpentine, but continued in small globules, which attached themselves to the bottom of the vessel.

(6.) It dissolved immediately in nitric acid, without effervescence, or any apparent change in the liquid.

(7.) When dropped into sulphuric acid, a copious effervescence took place, the smell of oximuriatic acid became evident, and the liquid remained clear.

(8.) When left in an open vessel, it evaporated completely, leaving only a green trace.

Not an oil.

Resembles the pyro-acetic spirit.

From these properties, it is obvious, that the liquid in question does not belong to the class of oils. It is a substance of a nature quite peculiar, and seems to consist of the two gasses simply combined together. It has considerable resemblance, at least in taste, to the pyro-acetic spirit of Mr. Chenevix.

Detonated with oxigen.

4. When olefiant gas is mixed with thrice its bulk of oxigen gas, it detonates very loudly, when an electric spark is passed through it, and burns with a strong white flame. According to Mr. Dalton, it consumes exactly thrice its bulk of oxigen gas, and forms twice its bulk of carbonic acid. My experiments, as will appear from the following

following table, very nearly coincide with his. The gas used contained 16 per cent of common air, and the oxigen gas was mixed with 11 per cent of azote.

Results.

	Measures of olefiant gas.	Measures of oxigen gas.	Residue after combustion.	Do. washed in lime water.	Measures of nitrous gas added to do.	Residue.
1	20	59	45	10	42	44
2	20	60	45	9	23	27
3	20	57	42	8	23	27

From this table we easily deduce the following:

	Measures of Gasses mixed.				Carbonic acid formed.	Residue.	Composition of Residue.	
	Pure olefiant.	Pure oxigen.	Azote.	Total.			Azote.	Oxigen.
1	16·8	53·14	9·06	79	35	10	9·06	2·9
2	16·8	54·04	9·16	80	36	9	9·16	1·8
3	16·8	51·34	8·86	77	34	8	8·6	1·4

This table obviously furnishes us with the following results:

	Olefiant gas consumed.	Oxigen gas consumed.	Carbonic acid formed.	
1	16·8	50·24	35	
2	16·8	52·24	36	
3	16·8	49·94	34	
	16·8	50·8	35	Average.
	100	302	208	Aver. per cent.

From

Its composition. From these experiments, it is easy to deduce the co sition of olefiant gas: 100 cubic inches of it, at the perature of 60°, and when the barometer stands inches, weigh 29·72 grains.

Of the 302 cubic inches of oxigen gas consumed, went to the formation of carbonic acid. The rema 94 cubic inches must have gone to the formation of w and they must have combined with a quantity of hidr which, if in the gaseous form, would have amounte 188 cubic inches. Therefore, 100 cubic inches of ol gas are composed of the carbon in 208 cubic inches of bonic acid, and a quantity of hidrogen equivalent to cubic inches.

Now, the carbon in 208 inches of carbonic weighs - - -	26·98 grai
108 inches of hidrogen gas weigh -	4·80
Total,	31·78
Weight of the olefiant gas, - -	29·72
Surplus, -	2·06

Thus the weight of the constituents found by an exceed that of the olefiant gas by about $\frac{1}{15}$th part;—a proof, that olefiant gas contains no oxigen. The scien chemistry, in its present state, admits of no stronger p than what we have now given.

It follows from the preceding analysis, that olefiant composed of about

Carbon, - -	85
Hidrogen - -	15
	100

Mr. Dalton considers it as composed of an ato carbon and an atom of hidrogen. This comes tole near to the preceding analysis, if we suppose the weig an atom of carbon 4.5, and that of an atom of hidrog For 85 is to 15 very nearly as 4·5 is to 0·8.

Olefiant gas detonated with less than its weight of oxigen.

6. There is a curious experiment, first made by Cr shank, and afterward repeated by Berthollet, whi thought it worth while to verify. When olefiant g mixed with less than its bulk of oxigen, and the mixt

fired by electricity, a quantity of charcoal precipitates, and the bulk of the residue after the detonation is much greater than before. The following table exhibits the result of my experiments in this way:

	Measures of olefiant gas.	Measures of oxigen.	Residue after combustion.	Do. washed in lime water.	Measures of nitrous gas added.	Residue.
1	39·5	28·5	100	94·5	37	121·5
2	40	30	110			
3	40	30	110			
4	40	29	110			

In each of these experiments, a quantity of charcoal was precipitated. It remained long suspended in the gaseous residue, quite dry, and made its escape into the air, if the detonating tube was turned up. I attempted to ascertain the weight of this charcoal, but was not successful. It was necessary, in order to collect it on the filter, to moisten it in the first place; and it was found impossible to dissipate the whole of the water, without altering the filter. Hence it was always a great deal too heavy, never weighing less than $\frac{1}{5}$th of a grain. Charcoal obtained.

From the preceding table we easily deduce the following:

	Pure olefiant gas.	Pure oxigen gas.	Azotic gas.	Total.	Carbonic acid formed.	Residue.
1	33·2	26·64	8·16	68	5·5	94·5
2	33·6	27·98	8·42	70		
3	33·6	27·98	8 42	70		
4	33·6	27·08	8·32	69		

It

In the first experiment, 5.5 measures of oxigen ga
to the formation of carbonic acid, and 3·74 were foun
combined, after the explosion, by the test of nitrou

New inflammable gas formed.

Hence it follows, that 17·4 measures of oxigen gas,
bined with 33·2 measures of olefiant gas deprived of
tion of its carbon, and formed a new inflammable
amounting to 82·7 measures in bulk, or almost doub
bulk of the two gasses that went to form it.

This examined.

The new inflammable gas being examined by a nev
ture with oxigen gas, and a new detonation, was fou
consume 73 per cent of oxigen gas, and to form 5
cent of carbonic acid.

Hence the 82·7 measures would have formed -
But 33·2 measures of olefiant gas would have formed

Difference,

From this it would seem, that about $\frac{1}{4}$th of the c
removed by the first explosion is converted into ca
acid, while $\frac{1}{4}$ths precipitate in the state of a black po

33·2 cubic inches of olefiant gas weigh	9·87 gra
The carbon in 23·5 cubic inches of carbonic acid weighs - - - - - - - - - -	3·05
Residue,	6·82
17.4 cubic inches of oxigen weigh - -	5·31
Hencce the weight of 82·7 cubic inches of the new gas cannot exceed - - - - -	12·13

Hence 100 cubic inches of it would weigh 14·66 g
and its specific gravity cannot exceed 0·4808.

This is on the most unfavourable supposition, th
water whatever is formed during the first combustio
water be formed, it is obviously lighter than we have
it. It is clear, therefore, that this new-formed gas, to
the name of oxicarburetted hidrogen may with pro
be given, is quite different from carbonic oxide ga
specific gravity of which is 0·956, or almost double
new gas.

9·87 grains of olefiant gas are composed of 1·49 hidrogen.
8·38 carbon.

9·87

Hence our oxicarburetted hidrogen gas is composed of - - - - - - - - - 1·49 hidrogen. Its composition.
5·33 carbon.
5·31 oxigen.

12·13

or per cent of carbon 43·9
oxigen - 43·8
hidrogen 12·3

100·0

It is not improbable, that this oxicarburetted hidrogen gas is composed of an atom of carbon, an atom of oxigen, and an atom of hidrogen. If this supposition be well-founded, the proportion of oxigen must exceed a little what we have obtained by our analysis. This would probably have been the case, if we had founded our analysis upon any of the succeeding experiments, rather than the first of the preceding table.

The preceding experiments, I flatter myself, entitle us to conclude, that *two* gaseous compounds of hidrogen and carbon exist. To the first we may give the usual name of carburetted hidrogen; to the second the name of supercarburetted hidrogen, as it contains very nearly twice as much carbon as the first gas does. There exists also a gaseous compound, consisting of oxigen, carbon, and hidrogen; but it differs in its properties from all other inflammable gasses hitherto examined. The reason why the inflammable gasses from vegetable and animal substances differ so much from each other is, that they usually hold an oil in solution, and are mixed with variable quantities of carbonic oxide gas. Conclusions.

II. *On*

II.

On a new Compound resulting from the Union of Benzoic Acid with Camphor. In a Letter from F. R. GOWAN, *Esq.*

To MR. NICHOLSON.

SIR,

SHOULD the following communication prove worthy of insertion in your Journal, it will give me great satisfaction if I find, that I can tend in the least to the advancement of chemical knowledge, by the relation of a few experiments and observations on a new oleaginous compound.—The experiments which I particularly wish to submit to your consideration are to illustrate the formation of this substance which I have recently produced from certain admixtures of benzoic acid, camphor, alcohol, and water; and which may also be produced in a similar manner from the sulphuric and nitric æthers, when substituted for alcohol.

New oleaginous compound.

By making solutions of benzoic acid and camphor in either of these solvents, I found, that, on the addition of a definite quantity of water, no precipitation of the acid or camphor was produced, but an oleaginous compound was formed. This seems to present an anomaly inconsistent with analogy; inasmuch as it proves, that water does not act primarily in causing a precipitation of these bodies either from alcohol or æthers, a circumstance, which a priori would not have been expected; but causes rather the formation of a triple compound, the production of which is increased in the inverse ratio to the purity of the alcohol employed.—From this preliminary sketch, it appears, that water becomes necessary in the ratio of the purity of the alcohol; or to speak generally, that any given quantity of common spirit of wine would require one part, while alcohol of the sepcific gravity of 0·800 would require double to effect the same end.

Solution of benzoic acid and camphor forms an oleaginous compound, on the addition of water in a certain proportion.

The ternary compound, however, resulting from any one of these fluids, viz. alcohol or æthers, consists of benzoic acid and camphor, which are brought into union, and preserve

This compound a benzoate of camphor,

served in this state, through the medium of either of the above-mentioned solvents: therefore the union of these two bodies furnishes a very good illustration of the old chemical law:—"*Corpora non agunt, nisi sint soluta.*" But, however, this axiom has now become obsolete, and rendered totally inconsistent as to its general application, from our present and more refined laws of chemical affinity; yet, nevertheless, I think it may be considered in the present combination as one fully applicable, because it illustrates the formation of the compound. Indeed, I shall have occasion to show, by the following experiment, that, if the connecting medium or solvent of these two bodies be destroyed by increasing the quantity of water, a subsequent diminution of power of that medium as a solvent must inevitably result; and the benzoic acid will be precipitated in crystals, while the camphor, the remaining constituent, will rise to the surface of the fluid.

which will not form if the water be in too large proportion.

Experiment 1.—Twenty grains of benzoic acid, and the same quantity of camphor, were mixed in half an ounce of alcohol, which readily dissolved them without the least change; but immediately on the addition of the same quantity of water, the substance in question was produced, accompanied with a sensible evolution of caloric. This production of heat, however, must be well known to depend on the powerful attraction, which the alcohol and water mutually exert to enter into chemical combination; and may be said to depend, according to Berthollet, on the condensation of volume*.

Exp. 1.—Benzoic acid and camphor dissolved in alcohol, and water added.

On allowing this solution to remain quiet, it gradually separated into two strata; the uppermost of which was a spirituous solution of a pale yellow colour: and the lower one was the new combination.

The fluid separated into two strata.

The properties of the upper stratum are as follows: It was of a pale yellow colour: (owing, no doubt, to a quantity of resin, which the acid contained, for if the acid be perfectly pure, both the oleaginous compound, and the solution above it, are perfectly transparent.) It was acid, and when exposed to a gentle heat in a retort, a spirituous solution, containing a quantity of camphor, came over,

Properties of the upper.

* Berthollet, Chem. Stat. v. i.

which was separated in small flakes on the addition of water; while the acid remained in the retort, and precipitated on cooling in a crystallized state.

Properties of the lower.

Properties of the lower stratum, or oleaginous compound.—It was of a bright yellow colour, depending, as I have before remarked, on a quantity of resin, which the acid contained. It is strongly acid, and has a pleasant aromatic smell. When a few drops were put upon a spatula, and exposed to the heat of about 250°, it was rapidly volatalized; and on taking it away, the remaining portion crystalized upon the spatula, like rays diverging from a centre. These crystals were dissolved in alcohol, which then reddened vegetable blues. When this peculiar compound was immersed in cold water, it was immediately decomposed, the alcohol entered into chemical combination with the water; the benzoic acid was precipitated in small shining crystals, and the camphor remained on the surface of the fluid. This then is sufficiently evident, to elucidate that water, although necessary to its formation, will not preserve it when formed, without the necessary admixture of a definite quantity of alcohol.

Boiling water readily dissolves it, and precipitates on cooling about $\frac{12}{16}$ths of the acid, in small flaky crystals; while the camphor is principally separated, and rises to the surface of the fluid, leaving, however, a small portion in solution. It is very soluble in alcohol, and, if dissolved in sufficient quantity, may be recondensed into the original compound. When mixed with a solution of the subcarbonate of potash it was decomposed, a benzoate of potash was formed, and a quantity of carbonic acid gas was evolved; while the camphor, the other constituent, was liberated, and floated on the surface of the fluid. In order, therefore, to obtain a quantity of the benzoic acid in a pure state, the camphor was separated from this solution by passing it through a filter, and obtaining the solution of benzoate of potash in the receiver. To a portion of the solution thus collected, sulphuric acid was added to saturation, which, from its greater affinity, formed a sulphate of potash, to the exclusion of the benzoic acid, both of which, however, were mechanically combined, and formed a white precipitate:

precipitate: this precipitate was afterwards thrown on a filter, and washed with a weak solution of alcohol in water. After passing through the filter, and remaining some time, crystals of the acid were formed. When to another portion of the same solution (benzoate of potash) heated, sulphuric acid was added, a very peculiar change ensued, viz. the benzoic acid was immediately precipitated in beautiful flaky crystals, and left the sulphate of potash in solution.

Exper. 2.—In trying how much benzoic acid and camphor was necessary to the formation of this compound, I found, that from the following proportions none of the oleaginous substance was produced.

Exp. 2.—None of the compound produced when too much of the menstruum is present.

Benzoic acid gr. xx, camphor gr. xx, alcohol an ounce, water the same. It will appear from this experiment, that the combination is only produced, when the substances which are necessary to its formation are in a certain ratio with respect to the quantity and purity of the alcohol employed; for it seems, that when they are so much diluted with alcohol and water, the affinity of the benzoic acid for the camphor is counteracted by the superior attraction of the alcohol for this body; and thus the benzoic acid and camphor are preserved in solution, without being able to exert their mutual affinities to produce the new combination.

In repeating these experiments in larger proportions, I observed some crystals, varying from half an inch to an inch in length, were formed (on standing for several hours) in the fluid above the oleaginous compound, and which I thought, from their magnitude, were composed of benzoic acid and camphor. It may also be proper to remark, that crystals are formed in the oleaginous substance, but much inferior in their magnitude to those which are found in the supernatant stratum.

Crystals formed.

Examination and properties of the crystals.—They were of an irregular, oblong, and flat shape; acid, partially soluble in cold water, but perfectly so at the temperature of 212°. When dissolved in a solution of the subcarbonate of potash, a quantity of carbonic acid gas was evolved; but no camphor was separated. When dissolved in alcohol, they gave to it acid properties; and on the addition of wa-

Their properties.

ter, a small quantity of benzoic acid was separated. This seemed a second confirmation, that camphor did not enter into their composition. From this examination, however, I think it is extremely probable; especially since we find the larger crystals are formed in the upper stratum, that camphor does enter into their composition; but that their increased magnitude is owing to the reciprocal union of the alcohol and water in combination with the benzoic acid.

From the two above-mentioned experiments we learn, that water is only essential to the production of the compound, inasmuch as it combines in definite ratios with the alcohol, and therefore acts as a condensing agent, whereby the particles of the acid and camphor are brought nearer together: while they in their nascent state (if I may be allowed the expression,) preserve their attractive quantities of alcohol, by which they are enabled, from their natural affinities, to enter into absolute union, and thus effect the formation of the oleaginous compound; or what may be more properly denominated a benzoate of camphor and alcohol.

It must be sufficiently obvious, from what has been above stated, that alcohol acts a very material part in the production of this compound; I therefore was induced to try, from their analogous power as solvents for these bodies, the effects of the sulphuric and nitric ethers.

Exp. 3.—Sulphuric ether employed instead of alcohol.

Exper. 3.—Twenty grains of benzoic acid, and the same quantity of camphor, were dissolved in half an ounce of sulphuric ether. To this solution the same proportion of distilled water was added, which produced at first a general turbidness; but the æther, from its not possessing much affinity for water, together with its superior levity, soon separated, and occupied the upper stratum, which on agitation had an oleaginous appearance. The result of this combination, however, seems nothing more than a separation of the ether by the mechanical admixture of water; the oleaginous appearance produced on agitation being caused by the mutual and equal repulsion of their particles, which thus give the spherical appearance to the compound. The density of this compound may be increased by augmenting the proportions of benzoic acid and camphor. Its princi-

pal

pal properties are its specific levity, the retention of its oleaginous appearance in hot water, and finally its total decomposition in cold water.

Exper. 4.—The same proportions of benzoic acid and camphor were dissolved in half an ounce of nitric ether: the colour in this combination, however, soon changed to a reddish brown, and a quantity of an oleaginous substance of the same colour was produced, without the admixture of any water. It is probable, I think, that the colour in this combination was produced from the action of the ether on a quantity of resinous matter in the acid. From a subsequent experiment, however, precisely in the same proportion, there was scarcely any of this compound produced; but on the addition of half an ounce of water there was a copious production of it, which gravitated to the bottom of the phial. After standing for several hours, a number of crystals had formed within it, and it had become much denser, and resembled in appearance melted phosphorus. Thus we see that water greatly facilitates the formation of the compound, and also causes by dilution the subsequent formation of crystals. When these two experiments were repeated with a purer acid, in the same proportions, analogous results were produced; but the compounds were of a pale white colour: and thus, I think, proving satisfactorily, that the colour in the former experiments was owing to the impurity of the acid. The general properties of this compound are its greater specific gravity; its spontaneous formation without water, and its immediate production on the admixture of this fluid; together with the resulting formation of crystals, which I suppose are produced in the ratio to the power of dilution. It is acid, and also preserves its oleaginous appearance in either hot or cold water, and is partly dissolved. If, however, it be put into too large a quantity of either, it is extremely probable that it will be dissolved or decomposed; because the power of attraction, which will preserve it in a small quantity, will be totally annihilated by a greater.

Exp. 4.—Nitric ether.

Thus after observing the peculiar combinations of benzoic acid and camphor with alcohol and ethers, I was induced farther

farther to make a series of experiments on the different vegetable, animal, and mineral acids, in order to observe whether any other acid or acids were capable of effecting a similar combination: as there seemed from analogy just ground to suppose, that any other mild vegetable acid would produce an analogous compound, when treated in a similar manner.

Experiments with camphor and other acids instead of the benzoic.

EXPERIMENTS.

5. Oxalic acid gr. xx, camphor gr. xx, alcohol ʒss, water ʒss
6. Tartaric acid - - - - - -
7. Boracic acid - - - - - -
8. Citric acid - - - - - - -
9. Camphoric acid - - - - - -
10. Succinic acid - - - - - -
11. Alcohol ʒss, camph. gr. xx, Lactic acid ʒss, water ʒii
12. - - - Formic acid ʒii -
13. - - - Acetic acid - -
14. - - - Prussic acid - -
15. - - - Phosphoric acid -
16. - - - Muriatic acid -
17. - - - Sulphuric acid -
18. - - - Nitric acid * - -

None of them produced such a compound.

From all these experiments, however, not one of the acids was capable of producing the substance in question but on the contrary caused in some instances the precipitation of the camphor previous to the addition of water. Benzoic acid, therefore, may be considered as exerting a very peculiar attraction to enter into combination with camphor, when assisted by the cooperative power of alcohol or ether. It is extremely probable, that the incapability of the above mentioned acids of forming compounds with camphor may arise in general from their being stronger acid

* Although nitric acid in this combination was incapable of producing an oily compound, yet in its undiluted state it readily dissolves camphor, forming that substance which has been improperly termed the oil of camphor, and which in my humble opinion better deserves the appellation of nitrate of camphor; since it would do away the ambiguous and unscientific term of the former, and convey, I trust, a better expression of its composition.

so as from their superior acid property totally to destroy that attraction which the alcohol exerts for the camphor; and thus, by altering the properties of the alcohol, destroy the effect of combination.

The readiness with which the oleaginous substance may be formed when benzoic acid is used may probably depend on the three following causes. 1st, That the benzoic acid naturally possesses a strong attraction to combine with camphor. 2ndly, That its attraction for alcohol is greater than that of any other acid. And 3dly, Through the medium of the alcohol, it is brought into actual combination with the camphor, so as to produce the oleaginous compound, or benzoate of camphor and alcohol. From these three causes I shall offer the following theory of its formation. As water is the principal agent in the production of this compound, it becomes necessary to consider its specific action in producing it; and first to consider its power as a solvent.—It must be well known, that water can only dissolve about $\frac{1}{10}$ part of benzoic acid; and from some recent experiments, which I have lately made on the solubility of camphor in distilled water, I find, that only a grain of this substance is soluble in two ounces, or 1624 troy grains; from which I infer its very slight power as a solvent for either of these bodies. It becomes, therefore, extremely probable, that the water exerts almost the whole of its affinity to enter into chemical combination with the alcohol, which we know from dilution would render it less capable of holding these bodies in solution. Thus it is reasonable to expect, that a quantity of benzoic acid and camphor will be separated from the alcohol, in proportion to the quantity of water added: is it not, therefore, probable, that they, (as I have before remarked) at the instant of their separation, preserve their attractive quantities of alcohol, through the medium of which they are enabled to unite; and thus finally effect the production of the oleaginous compound?

Theory of the formation of the compound.

I cannot, however, conclude this communication without entreating my reader to excuse the unfinished manner in which I have offered these few experimental facts and observations, from believing the relation of them may probably be the means of exciting some gentleman to examine the

subject with more accuracy and precision, so as, I trust, to confirm in a more satisfactory manner the experiments, which I have ventured to bring forward, and achieve the principal object of the communication.

I am, SIR,

Your humble Servant,

S. B. H. F. R. GOWAR.

March 23, 1811.

III.

Comparative Analysis of Gum-Resins: By MR. HENRY BRACONNOT, *Professor of Natural History, &c.*

(Concluded from p. 237.)

ART. V. *Analysis of Olibanum.*

Action of heat on olibanum.

§ I. OLIBANUM is difficultly melted; and when kindled by the flame of a candle continues to burn of itself, leaving white ashes.

Distilled with water,

Twenty gram. [308·8 grs.] distilled with water yielded about 1 gram. [15·4] grs.] of volatile oil; of a pale lemon colour, and the smell of which is not unlike that of lemons.

and dry.

A similar quantity distilled with the naked fire gave a very large quantity of a brown empyreumatic oil, and an acid watery liquid in no great abundance. These products did not give any perceptible indications of ammonia on the addition of lime; but on bringing a little muriatic acid near the mixture some slight white fumes were visible, which appear to indicate the presence of volatile alkali. In the retort remained a compact coal, weighing 2·5 gram. [38·6 grs.] which left after incineration 55 cent. [8·5 grs.] of ashes, composed of 5 cent. [0·77 of a gr.] of potash partly saturated with sulphuric, muriatic, and carbonic acids; 6 cent. [0·93 of a gr.] of phosphate of lime; and 44 cent. [6·8 grs.] of carbonate of lime.

Heated with alcohol.

§ II. *A.* 25 gram. [386 grs.] of powdered olibanum were heated with a sufficient quantity of alcohol, and filtered. On the filter remained a pretty copious whitish substance

stance, which, being well washed with alcohol and dried, weighed 9 gram. [139 grs.]

B. This matter insoluble in alcohol was dissolved in boiling water, with the exception of a soft grayish substance, of a gummy appearance, and weighing when dry 1·3 gram. [20 grs.]. This burned with flame; and when treated with nitric acid, produced a greenish resiniform substance; which may lead us to suspect, that some resin had escaped the action of the alcohol, though I took great care to wash the residuum with boiling spirit of wine. Residuum dissolved in water.

C. The aqueous solution *(B)*, after being filtered, yielded by evaporation 7·5 gram. [115·8 grs.] of a gum, of which the following are the properties. Gum.

1. This gum, which was considered as extractive matter by the older chemists, though it has by no means the appearance of it, has a yellowish transparency, and a flat taste. Its properties.

2. It dissolves easily in water, without leaving any residuum.

3. Exposed to the fire it burns with but little flame, and leaves white ashes, formed in great part of carbonate of lime.

4. Its aqueous solution does not redden infusion of litmus.

5. Oxalate of potash forms a precipitate in the solution.

6. Acetate of lead produces in it no very apparent change; but the nitrate, and more especially the subacetate of lead and the nitrate of mercury occasion in it very copious, thick, white precipitates, completely soluble in distiled vinegar.

7. The decoction of galls precipitates the solution of the gum of olibanum, but not that of the gum of myrrh.

8. Lime-water in excess does not affect its transparency, even after standing some time.

9. This gum is carbonized by sulphuric acid, but does not emit acetic vapours on heating the mixture. What then is it, that neutralizes the lime in this gum? I had a suspicion of benzoic acid, but this requires to be confirmed.

10. The gum of olibanum heated with nitric heat lets fall, particularly on cooling, a pretty large quantity of very white powder, granular as if crystalline. On continuing the

the evaporation to dryness, a residuum was obtained, which when well washed and dried, furnished mucous acid amounting to a third of the weight of the gum employed The waters of elutriation contained oxalic and malic acid but in small quantity.

Resin. *D.* The alcoholic solution (*A*), evaporated to dryness yielded 14 gram. [216·2 grs.] of resin; whence it follows that there was a loss of 2 gram. [30·88 grs.], which ascribe in great part to the volatile oil.

Its properties. 1. The resin of olibanum is of a reddish yellow colour it easily cracks on cooling, becomes very brittle, has n perceptible taste, is electric when rubbed, and a good de resembles common resin.

2. It softens in boiling water, but it requires a greate heat to melt it. Exposed to flame, it burns, and emits smell that is not unpleasant.

3. This resin, heated with a solution of caustic potash and evaporated to dryness, left a residuum not very solu ble, which, after being well washed, was diffusible in boil ing water, producing an emulsion. Common resin unite much more readily with alkalis *.

4. Sulphuri

* Resinous substances have never yet been examined with th minuteness and attention they deserve, as appears from the fe facts collected respecting their chemical properties, and even c these several are inaccurate. Many even suppose, that alkali have no action on resins; as a trial of which I made the followin experiments:

Action of alkalis on common resin. Into a cold and very weak solution of potash I put commo resin powdered, by a little at a time. The result was a complet soapy solution, which became thick and ropy like the white of a egg. The weakest acids occasioned in it a copious white sedimen Alkalis and neutral salts do the same, in consequence of the greater affinity for water. A large quantity of common water to decomposes this soap, and the precipitate it occasions is but partl soluble in alcohol. The part that refuses to dissolve in it still con tains a great deal of resin united with lime.

If muriate of lime be added to this resinous soap, the mixtur thickens into a whitish mass resembling pap, consisting of resi and lime.

All metallic solutions completely decompose the soap of resin and several of the precipitates thus formed may be employed i painting.

Fift

4. Sulphuric acid dissolves the resin of olibanum without heat, forming a red solution, from which water throws down a white precipitate. If it be heated some time, water separates from it a black resin, soluble in nitric acid; which by evaporation converts it into a brown residuum of an astringent taste mixed with bitterness, and the aqueous solution of which precipitates gelatine, but not sulphate of iron.

5. If the resin be treated to dryness with eight parts of of nitric acid at 38°, it is almost wholly converted into the resiniform matter, the properties of which I have already made known. The waters of elutriation, which contained no oxalic acid, yielded by evaporation a residuum, which, when heated afresh with nitric acid, afforded none of the tanning matter discovered by Mr. Hatchett; but a bitter, soluble substance, partly precipitated on cooling from its solution in boiling water, soluble also in alcohol, and producing precipitates in several metallic solutions. Converted by nitric acid into a kind of resinoamer.

This substance appeared to me to possess properties analogous to those of what I have termed resinoamer, but it differs from it in the stability and proportion of its elements.

Art. VI. *Analysis of Gum Ammoniacum.*

§ I. Gum ammoniacum, exposed to a heat not sufficient to decompose it, softens easily, and loses 6 per cent of its weight. Action of heat on ammoniacum.

Twenty-five gram. [386 grs.] distilled with a naked fire yielded 12 gram. [185·3 grs.] of liquid, the greater part of

Fifty gram. [772 grs.] of resin, heated to saturation with solution of potash, dissolved entirely; and by evaporation were obtained 69 gram. [1065 grs.] of solid resinous soap, dry at a low temperature, of a brown colour, and which I found to be perfectly similar to Starkey's soap. Baumé and Legendre therefore had reason to presume, that only the thick and resinous part of oil of turpentine could really combine with potash; for this alkali acts on volatile oil only by promoting its conversion into resin to unite with it afterward. Starkey's soap.

Volatile alkali, diluted with a great deal of water, also combines with resin very readily, and quickly reduces it to the saponaceous state. This compound diluted with water acquires after a time the thickness of starch.

which

which was a brown empyreumatic oil. This product emitte a smell of volatile alkali on the admixture of lime. In th retort was left a coal, weighing 7·5 gram. [115·8 grs.] which after incineration left 3·2 gram. [49·4 grs.] of ash mixed with gravel. From these weak nitric acid extracte 1·5 dec. [2·3 grs.] of phosphate of lime, and 3 dec. [4· grs.] of carbonate of lime.

Ammoniacum boiled in alcohol.

§ II. (*A*) I boiled 25 gram. [386 grs.] of powdered am moiacum with a sufficient quantity of alcohol at 36 [sp. gr. 0·837]. The solution filtered boiling let fa nothing on cooling. A white substance remained on th filter, which, after being washed with alcohol and dried weighed 5·8 gram. [89·5 grs.].

Residuum dissolved in water.

B. This substance, being heated with water, dissolved i it in great part. The solution, being strained through fin linen, left a grayish substance, soft and as it were glutinous insoluble both in water and spirit of wine, black after i was dried, and then weighing 1·1 gram. [17 grs.]. I burned more easily than a gum, and was reduced to gra ashes. Treated with nitric acid it gave a yellow colour and afforded a little oxalic acid.

Gum.

C. The aqueous solution (*B*) yielded on evaporation 4·6 gram. [71 grs.] of gum.

Its properties.

1. This gum was transparent, of a reddish yellow colour, and a slightly bitter taste, somewhat brittle, and dried easily in comparison with other gums.

2. It burned without any apparent flame, leaving some white ashes, which dissolved in acids with effervescence.

3. Water dissolved it entirely; and the solution could barely be said to redden infusion of litmus.

4. It was precipitated entirely from its solutions by the subacetate of lead; but not at all by acetate, or the nitrate of lead or of mercury. The latter merely rendered the solution milky.

5. Lime-water in excess does not render the solution of this gum at all turbid, therefore it contains no phosphoric acid; yet its ashes yield a little phosphate. Hence I conceive phosphorus exists in this gum in a state of combination with the other elements that compose it.

6. Oxalate

6. Oxalate of ammonia throws down from the solution a precipitate of oxalate of lime, but I do not know by what acid the lime is neutralized in the gums*.

7. Decoction of galls detects nothing in it.

8. This gum, treated with the nitric acid of the shops, yielded nearly the same products as that of olibanum; that is, a large quantity of mucous acid, some oxalic acid, and a very little malic acid.

D. The alcoholic solution (*A*) being evaporated to dryness, yielded 17·5 gram. [270 grs.] of a resin, which exhibited the following characters. Resin.

1. It was of a reddish yellow, transparent, brittle as wax at a low temperature, having a wavy and shining fracture, receiving a slight impression from the nail, and readily growing soft in the mouth or between the fingers. It had no sensible taste: its smell was the same as that of ammoniacum itself. Friction did not render it electric. Its properties.

2. This resin, like wax, melts at 43° [129° F.]. At a higher temperature it swells up considerably, emits a peculiar smell, and leaves a light, spongy, bulky coal.

3. This resin easily combines with alkalis, even cold; and the results are saponaceous compounds of considerable bitterness †.

4. Sulphuric acid readily dissolves the resin of ammoniacum, particularly with a gentle heat. Water decomposes this solution. If it be heated more, the result is a hidroguretted coal; which, after being washed, dissolves in nitric acid, and yields by evaporation an astringent substance, which gives a brown precipitate with isinglass.

5. Nitric acid heated on the resin at first produced only some white vapours: but at length the red fumes appeared on a sudden with great vehemence. The product was a Treated with nitric acid

* Mr. Vauquelin, who has made known the existence of phosphate of lime in the ashes of gum arabic and gum tragacanth, supposes the lime in these gums is saturated in great part by the acetic acid.

† The soap or saponule of common resin has likewise a decidedly bitter taste, which seems to me to indicate in these resinous compounds efficacious properties, that deserve the attention of the observing physician.

yellow

formed a resinoamer.

yellow resiniform substance, which dissolved during t process. On evaporating to dryness I obtained a resino mer substance of a very pure yellow, fusible at a gent heat, uniting with alkalis, soluble in alcohol and in boi ing water, and falling down in part from the latter cooling; soluble in part too in a large quantity of co

Fine yellow dye.

water, and communicating to its solutions a very fine yello colour, which adheres very tenaciously to the fingers, a is easily fixed on wool and silk, giving them a fine lust unalterable by oximuriatic acid, and even by weak alkali consequently there is nothing that can be compared with for permanence among the majority of dyeing drugs. may add too, that this yellow appears to me to merit t preference before others in point of cheapness, consideri the small quantity of it required.

This substance, mixed with gum, might yield a mo pure and permanent yellow than gamboge.

Component parts of ammoniacum.

From this examination of gum ammoniacum it follow that 100 parts are composed of

Gum - - - -	18·4
Resin - - - -	70
Glutiniform matter	4·4
Water - - - -	6
Loss - - - -	1·2
	100

IV.

On the Mechanism of Respiration in Fishes: by Mr. C. Duméril, *Professor at the Medical School, &c.*

The mechanism of respiration in fishes similar to that of some reptiles.

IN these papers I intend to explain some ideas respecti the act of respiration in fishes; to demonstrate, that t mechanism by which water is drawn into the mouths these animals is precisely similar to that of the respiration some reptiles; and that, in consequence of this mode respiration, the seat and perception of the organ of sm

* Mag. Encyclop. Nov. 1807, p. 35. Read to the Instit August the 10th.

in fishes are altogether different from what they are in other vertebrate animals.

In the mammalia and birds the whole of the venous blood is under the necessity of coming to place itself in contact as it were with the air, that penetrates their lungs. These organs are enclosed, and capable of expanding, in the thorax; a cavity, the bony sides of which are capable of various motions, and particularly of those that tend to increase or diminish its diameters. Respiration of beasts and birds.

In the mammalia the thorax appears particularly intended to protect the principal organs of the circulation; and to determine the motions necessary to respiration. In all other vetebrate animals, that have lungs, these parts are contained in a less determinate space, with the organs of digestion and other viscera. Seat of the lungs.

The ribs, or bony hoops, that surround this cavity, are evidently levers, that mechanically produce the expansion or contraction of the space surrounding the lungs. The powers that move the bones in these two directions, and which are the muscles of the thorax and abdomen, and the diaphragm, consequently determine the act of inspiration, as well as that of expiration. It was necessary to recapitulate these general principles, to give a clearer notion of what follows. Acting causes of respiration.

If it be true, as physiologists at present admit, that the ribs and muscles inserted in them are the chief mechanical agents of respiration; we must necessarily infer, that vertebrate animals destitute of ribs, or in which these bones cannot be employed to produce those motions, must have some other mode of breathing: and this in fact is the case. Where ribs are wanting a different mechanism necessary;

The first example we have in certain reptiles, as frogs, lizards, &c., during the first stage of their existence. These animals, previous to their metamorphosis, breathe in the manner of fishes, of which they have commonly the form, structure, and habitudes. They take in water at the mouth, and immediately expel it through a single bronchial aperture, or several holes in the side of the neck. Thus tadpoles are similarly circumstanced with fishes of the genus sphagobronchiæ of Bloch, or unibronchaperturæ of Lacépède; while the young lizards, protei, sirens, and other urodelæ, as in certain reptiles in their first stage, &c.

urodelæ, bear more resemblance to the sharks; particularly to that described by Schneider, and represented at fig. 31, in his edition of Bloch's System of Ichthyology; and which he has termed ciliaris, because a portion of the gills appears externally like a fringe.

The tadpole state sometimes long.

This period of the life of batrachian reptiles however is not so short, as has hitherto been supposed. Of this I have had an opportunity of satisfying myself, having kept tadpoles more than three years *. Yet when the lungs of these animals are completely formed, their gills lost, and they have only air to breath, there is hardly any thing altered on this account in their primitive organization, and they retain as we may say their old mode of respiration.

Reptiles with lungs, but no ribs, breathe in a peculiar way.

It is well known, that the batrachian reptiles have no ribs, or at most only the rudiments of these bones: the lungs therefore must be inflated by some mechanism different from that of other animals. This was conjectured by Herholdt and Rafn of Copenhagen, and almost immediately demonstrated by Cuvier and myself, though I have since corrected some of our former observations †. It is

Some reptiles slow in their changes.

* It has been said, that most of the batrachian species undergo their changes in one year: this is true with respect to the common frog, the edible frog, the green frog, the *salamandre abdominale*, and probably several others. It is known however, that the rana paradoxa is sometimes two years in the tadpole state; and I have observed, that the young marbled lizards retain their gills more than one year, having fed such in the spring. Besides, I have kept in my house for three years tadpoles of the *crapaud accoucheur*, which have constantly refused all kind of vegetable food, and which devoured one another, if they were not supplied with the flesh of animals.

Mode in which the nostrils are closed.

† There is neither valve nor valvulæ within or at the base of the nostrils. In all the anouri, that have the tongue free and its root attached to the cavity of the jaw, the two lobes apply themselves to the interior orifice of the nose, so as to prevent the issue of the air, or any other fluid contained in the mouth, at the time of deglutition. In the urodeli, which have no tongue, two tubercles, answering to the anterior bifurcation of the os hyoides, and covered by the inner membrane of the mouth, apply themselves to the two apertures of the nostrils, which are observed before the orbits of the eyes, on opening the mouth of any of the salamanders.

now

now known, that the respiration of the batrachian reptiles is effected by means of the muscles of the throat, and the elastic plate of the hyoides, which perform the office of ribs and diaphragm. And for inspiration thus to take place, the mouth must previously be closed; so that if an adult salamander or frog were placed in water, with the mouth kept open by force, or even left so in the moist and open air, it would soon die of suffocation. In all these animals the air is expelled suddenly from the lungs by the action of the abdominal muscles. It issues from the mouth in very large bubbles, and by a sort of vomiting, which explains the force and continuance of their croaking, even under water, as is observed in a great many species.

Tortoises.

All the chelonian reptiles are circumstanced precisely as the batrachian. Not that the tortoises for instance are destitute of ribs, or sternum, as no animals have these parts more solid and distinct. But all these ribs are united to the vertebræ and to each other, to form that bony shell, which covers their muscles, limbs, and viscera, in the back; as the parts of the sternum spread abroad cover them on the belly.

Mistake of Townson respecting their breathing.

I believe I first demonstrated in my public lectures, on living tortoises, the mistake of Townson in his examination of the mechanism of respiration in a tortoise, the under shell of which he had removed, and in which the mechanical action of the lungs was still exerted. This gentleman ascribes the faculty of drawing the lungs outward to two muscles, which can only compress them; while the motion of the throat, and the complete cessation of the act of inspiring when the mouth is open, would have given him a better insight into the true mechanism of this function, and the analogy that exists in this respect between tortoises and frogs. Indeed this is an observation, to which we are naturally led; and Mr. Cuvier, who has verified it, adopts the same opinion in his Lectures on Comparative Anatomy, vol. iv, p. 368.

Still these respire air, but by a different mechanism.

Here then we find the act of respiration performed in certain reptiles differently from what it is in animals that seem to precede them immediately in the scale of beings. Yet, though the mechanism is different, the effect is nearly

the same, since in both cases air is equally introduced into the lungs, there to produce and undergo a certain action. The real difference is only in the mode of inspiration, and the connexion of its continuance with the motion that empties the lungs. In fact, the air, inspired by small quantities in succession, is always introduced at the nostrils, and issues from the mouth at a single discharge of longer or shorter duration. During the stay of this large quantity of air inspired in the lungs, which seem to have been greatly expanded for the purpose, it divests itself of the oxigen it contains, so that the moment of expiration is to a certain degree arbitrary.

Difference of structure between these and fishes.

Most of the circumstances that have been mentioned will be exhibited at large in the following examination of the respiratory apparatus of fishes. All vertebrate animals, that breathe air alone, have only two apertures in the lower part of the mouth. One, intended for the passage of food, is the œsophagus: the other, admitting the passage of the air into and out of the lungs, is the glottis. Fishes on the contrary, without exception, have the throat perforated with four, six, or seven large apertures on each side, beside the passage leading to the stomach. These apertures supply the place of the glottis; and are commonly furnished on their edges with fringed appendages, which fit into each other, but are capable of being separated to a greater or less distance, to strain the water that passes through them. Each of these apertures directs the liquid into a space bordered by two bronchial laminæ, which are covered by a membrane, in the substance of which are commonly solid radii capable of motion, and a thin plate called the operculum, which can be moved so as to form a chink on each side. This aperture alone furnishes an issue to the water, which by staying on the bronchiæ has served for the respiration of the fish.

Manner in which fishes respire.

When a fish respires, it begins by opening the mouth, to suck in the liquid in which it is immersed. The water immediately comes in to fill the vacuum produced by the separation and protraction of the labial bones. The bottom of the throat is then raised, and the bronchial apertures frequently closed. But presently the lips approach each other;

other; all the hinder part of the head is enlarged by the raising of the opercula; the bony arches, that support the bronchial laminæ, are acted upon by the hyoides, which is depressed as in the act of swallowing; these arches separate from each other, and the water immediately enters into the spaces between them; but, at the moment these opercula are raised and separated from the body, the hyoides and all the bronchial laminæ connected with it rise against the arch of the palate, and drive before them all the water contained in the cavity of the mouth, which passes entirely round the bronchiæ, and is commonly discharged by the exterior apertures.

Thus we see the respiration of water by fishes is a real deglutition, but incomplete, because the mouth is perforated at bottom, and suffers the liquid, which necessarily enters with all its food, to escape.

This explains how fishes can swallow air in pretty large quantity *, because the specific gravity of gaseous fluids carries them to the highest part of the mouth, which is seldom perforated: this being the reverse of what happens in animals that respire air, which cannot naturally swallow it, because this fluid escapes by the posterior opening of the nostrils; while they can all swallow water without any difficulty. Fishes can swallow air.

The cetaceæ, which from their nature are obliged to reside in water, are circumstanced precisely the same as fishes. Every time these animals open their mouths to seize their prey, they are under the necessity of expelling the fluid introduced into them; and accordingly they are furnished with a peculiar organ, analogous in its effects to that of fishes. By the help of this organ they expel the water through their nostrils. Cetaceæ.

All the muscles, that serve in fish to expel the water from the cavity of the mouth into that of the gills, are the same as those that produce deglutition in other species of animals: only the apparatus is much more complex, as will Muscles of respiration in fishes.

* This commonly happens in the balistes and tetrodontes, which come to the surface of the water to suck in air, and thus puff themselves up considerably.

readily appear on comparing the structure of the os hyoid and bronchiostegous arches, which represent its horns, elongations.

The motion that produces the expulsion of the water altogether independent of that of the mouth and throa though it is its common consequence, and occurs alternat ly, and is nearly isochronous with it. The powers th occasion the expulsion of the water are equally situate abo the head; and it is chiefly this apparatus of bones and n merous muscles, that renders the heads of fishes so bulk for they are frequently more than a third of the whc length of the animal. The bronchiostegous arches, i cluded in the substance of the membrane, take the place the true ribs: and in those species that want these bon muscles in the shape of pouches fulfil the same offic acting on the water nearly in the same manner as the a ricles of the heart on the blood, which they propel into t ventricles; or as the muscular bags found in the spiracles whales.

This theory confirmed by apparent deviations.

The theory I have laid down seems to be confirmed ev by the anomalies, that occur in some fishes, in which spiration appears to be effected in a different way, a which consequently exhibit some slight modifications in organs.

Two peculiarities.

The common phenomena of the respiration of fishes ha already been described: but there are some, as the plagi stomes and cyclostomes among the cartilaginous fish which do not constantly take in water at the mouth; a others, in which respiration may be termed arbitrary in movements. These two peculiarities, which I shall ex mine somewhat at large, seem to show great analogy w the reptiles already noticed.

1st, in fishes, that frequently lie at the bottom of the water.

The rays, sawfish, and many of the squali, when th make no effort to sustain themselves in the water, natura sink to the bottom, and lie on the sand: and as their mou are in the under surface of the body, if they opened th like other fishes to draw in water, they would necessa suck up gravel and other foreign matters, by which th gills might be wounded. On this account they seem to able to breathe in the manner of frogs and adult salam

de

ders, nature having furnished them with two apertures at the top of the head communicating directly with the throat. These are real nostrils, placed behind the eyes, but formed to admit the water into the mouth, whenever the throat dilates. The return of the liquid in the same direction is prevented by valves; and the animal, performing the action of deglutition without opening the mouth or œsophagus, forces the water into the cavities of the gills.

2d, in fishes that adhere to others, or to solid bodies, by their mouths.

The mode of respiration too must be different, but for another reason, in the lampreys and gastrobronchi, the only species destitute of true jaws; and which have been classed perhaps erroneously among the vertebrate animals, from which they differ considerably, as I intend to show in a paper that I shall soon have the honour of laying before the class.

Many of these species are parasitical: they fasten themselves by the mouth to other fishes, or fix themselves on stones and other solid bodies by the help of this part and a kind of suction. The water therefore must necessarily enter by some other orifice, that of the mouth being closed. In fact we perceive on their head, or in their lips, a patulous orifice opening to a canal, which directs the water into the throat as often as a vacuum is produced in it. The motion of deglutition that follows forces this water to penetrate into peculiar cavities, in which the bronchiæ float; without this fluid being able to issue out by the same orifice, though Bloch ascribes this faculty to them, and, as well as Rondeletius, has figured this sort of fountain in several of the plates of his superb work.

These similar to reptiles of the frog kind.

All these fishes then are circumstanced precisely the same as the batrachian reptiles, since they inspire water at the nostrils, and by deglutition force the fluid into the pulmonary cavity, from which it is afterward discharged by a different orifice.

Peculiarities where respiration is in some degree arbitrary.

We find too some peculiarities of conformation in other species, the respiration of which is as it were arbitrary, at least as to the duration of the three periods, or movements, that compose its mechanism, namely inspiration, deglutition, and expiration. Besides, these circumstances produce,

duce, as we shall see presently, a result nearly similar t
that I have mentioned in the tortoise.

Fishes that live habitually in mud.

There are fishes that live habitually in mud and sand
some to lay snares for the species on which they feed,
the lophii, siluri, murenophides, &c.; others to shelt
themselves from the attacks of more voracious fishes, as t
ammodytes, callionymi, batrathoides, cotti, &c. In gene
in all these inhabitants of the mud we find the cavity of t
bronchiæ considerably expanded, in order to receive
larger quantity of water, which is kept as it were in r
serve: we perceive also, that the radii of the bronchios
gous membrane are considerably elongated, and the e
ternal aperture of the bronchiæ is very small in proporti
to the extent of the cavity. All these circumstances a
very naturally explained, when we know these speci
like frogs and tortoises, require several successive aspi
tions, to fill with water that kind of bag, in which t
bronchiæ are immersed, and which can be emptied at on
when the fish has no longer need of remaining in ambusca
or comes into pure water to inspire a fresh portion.

An additional organ in such as can live some time out of water.

We find an addition to the common organization in th
species of fishes, which in certain peculiar circumstan
can leave the water, and remain on land, or in the mud
half-dried ponds, for a larger or shorter time. I am n
speaking here of the false bronchiæ, which Mr. Brousso
has very well described under the name of pseudobronch
and which has since been found in several fishes; but of
peculiar organ, a vascular expansion analogous to lun
which lies loose in a cavity appropriated to it, but comm
nicating with that of the common bronchiæ, from whic
receives water. Consequently this is an accessory org
similar in its uses to the air-bags observed in the chamele
and in all birds. It is true we are yet acquainted with f
instances of this remarkable arrangement; but I sh
point out five, that are very striking.

Instances of this.

I shall first mention the *cephalopholis scansor* of Tr
quebar, [*perca scandens*], described by Daldorf in the
volume of the Linnean Transactions. This fish, whic
in the Museum of Natural History at Paris, has the facu
of climbing trees by means of the spines, with which
differ

different parts of the opercula are furnished. The *osphromenes goramy*, the organization of which has been described and figured from Commerson by de Lacépède, and which appears to possess the same organ, as far as may be judged from the structure and analogy. The *macropteronotus sharmuth*, the supplementary bronchiæ of which have been described by prof. Geoffroy in the 3rd volume of the Bulletin des Sciences. The *tetrodon Honkenii*, figured by Bloch under the name of *hérisson tigré*, which has exhibited the same organs to Mr. Schneider. And lastly the *hydrargyrus swampine* Lac., which Mr. Bosc found in Carolina, and of which he has given us some specimens for the Museum. In this fish I have just found these organs.

From the preceding facts it appears to follow, that the mechanical act of respiration in fishes is similar to what takes place in several reptiles, and that the motion of respiration is necessarily combined with that of deglutition. This connexion between the organs however need not surprise us, when we observe under other circumstances, and for another object, this same function of respiration necessarily combined in the reverse order with the act of deglutition. Thus the adult elephant cannot drink, if he do not make a strong expiration into his trunk at the moment of deglutition. Other functions might afford us many analogous examples; but these considerations, though highly important, would be misplaced here, and carry us too far from our subject.

Connexion between the mechanism of respiration and deglutition not extraordinary.

It occurs in the elephant.

V.

Farther Experiments on the alkaline Strength of the Product of the Oxigenation of Potassium, with Observations on the Nature of the alkaline Metalloids: by J. Murray, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON,

Edinburgh, April the 1st, 1811.

SIR,

In the communication inserted in the last Number of your Journal, I gave an account of some experiments, made with the view of ascertaining the comparative alkaline strength

Object to determine the comparative strength of

strength

common potash and that from potassium.

strength of the potash formed by the combustion of pot sium, and of fused potash prepared by the usual proce The facts, that fused potash contains a considerable p tion of water, and that in its decomposition by galvani there is not that evolution of hidrogen which might expected from the decomposition of this water, led me suppose, that hidrogen enters into the composition of p assium, and that the potassium in burning must form alkali containing water; and the most direct mode of termining this appeared to be the ascertaining, whether product of the combustion of potassium is the same or in alkaline strength with common ignited potash. As t is an investigation of considerable delicacy, and the pre determination of it difficult, I have since been occupied repeating and diversifying my former experiments.

Chief sources of errour.

The principal sources of errour, which attend the inv tigation, appear to arise from the difficulty of convert potassium into potash by combustion, without its acting the substance with which it is in contact; and from various degrees of oxidation of which it is susceptible, which are not easily determined.

Potassium acts on platina when burned on it.

In burning potassium on platina the latter metal is ac on, and a portion of its oxide remains mixed or combi with the potash formed. I have noticed the insoluble p der which remains, when the product of the combustion dissolved in water, and which is probably principally ox of platina. This substance having been collected and ca fully weighed, and the weight deducted from that of product which had been dissolved, it did not appear p bable, that it could give rise to any important erro Still it was desirable, if possible, to avoid its operati and I endeavoured therefore to find some other subst on which the potassium would not act, but without succ

So it does on china,

In burning it on chinese porcelain, the surface is erod and on afterward dissolving the alkali in water a light

and on silver;

latinouslike precipitate, probably silex, falls down. burning it on silver there is also a mutual action betw the silver and the potassium indicated by a small quantit precipitate, when the product of the combustion is solved in water. It appeared to me however, that

ac

action both on the porcelain and the silver is considerably less than that on the platina, and I employed them therefore in the following experiments. but less than on platina.

Seven grains of potassium were burnt on a silver basin. During the combustion a portion was dissipated in a dense white smoke, and the product did not exceed 7 grains; it was of a grayish colour, and fusible, remaining soft and nearly liquid, at a low red heat. Five grains of it were weighed while warm, and were dissolved in water. The solution was nearly transparent, depositing only a few flocculi. It was neutralized by an acid, composed of one part of nitric acid, by *weight*, and twenty parts of water, and of which 5 grains of ignited potash neutralized 139 grains *. The quantity required for neutralization, after separating the insoluble matter, and allowing as accurately as possible for its weight, was equivalent to 144 grains to 5 grains of the product free from this matter, and similar results, with differences of not more than 2 or 3 grains, were obtained on several repetitions of the experiment. Experiment with potassium burned on silver.

Seven grains of potassium were burnt on a cup of chinese porcelain. The product was lighter in colour than in the preceding experiments, being of a yellowish gray, nearly white. Five grains of it were weighed while warm, and dissolved in water. A flocculent gelatinouslike precipitate subsided, which, as it was found to be dissolved in a preceding experiment by a very weak acid, was removed before neutralization, dried, and weighed. The solution required for neutralization, of the diluted nitric acid, a quantity equal to 141 grains to 5 grains of the product free from this insoluble matter. Potassium burned on china.

These experiments then, in common with those I have formerly stated, appear to establish the conclusion, that the potash produced by the combustion of potassium is very little superior in the power of neutralizing an acid, to the common fused potash. It is with much caution that I have admitted this conclusion, as the opposite result has been supposed to be established, on the authority of some The product but little superior to common fused potash.

* The acid which I used in my former experiments was composed of one part of nitric acid by *measure* with 20 parts of water.

eminent

eminent chemists. But, after taking every precaution to guard against any source of fallacy, I have been able to obtain no other result. If the product of the combustion of the potassium were the real alkali, 5 grains of it ought to have neutralized 162 of the above acid, since 5 grains of ignited potash, containing at least 14 per cent of alkali, neutralized 139 grains.

But potassium may be oxided in different degrees.

The only circumstance which I can discover, as leaving any room for doubt, is that of the different degrees of oxidation, of which potassium is susceptible. There is reason to believe, that a compound may be obtained at a lower degree of oxidation than potash; and from the more recent experiments of Gay-Lussac and Thenard, the general result of which has been stated by Mr. Davy, it farther appears, that another compound, in which the potassium is more highly oxidated than it is in potash, may be formed. This last substance, they have stated, contains three times more oxigen than potash does; this excess of oxigen is expelled by exposure to a red heat, or when the product is dissolved in water, it is disengaged. If this substance had been the one operated on in the above experiments, it might give rise in part to the apparently equal alkaline strength of the potash formed by the combustion of potassium, and ignited potash; as a given weight of it would contain less real alkali than when this excess of oxigen was not present. But the circumstances connected with the experiment lead me to believe, that no important errour from this cause could have operated. The product of the combustion of the potassium was always kept for some time at a red heat, before it was operated on, by which, according to Gay-Lussac and Thenard, this excess of oxigen is expelled; and except in one experiment no effervescence was perceived on dissolving it in water.

This might have affected the result.

though it did not appear to be the case.

The experiments favour the opinion, that potassium is a compound of a metal with hidrogen.

If these sources of errour have not operated, and the experiments be correct, they appear to me to support the opinion I have advanced with regard to the nature of potassium. Were it the real base of the alkali, forming it when combined with oxigen, the compound thus formed must be superior in alkaline strength to common potash, which contains so considerable a proportion of combined water.

Excluding

Excluding the hypothesis of Gay-Lussac and Thenard, there appears to remain only one other view to be adopted—that potassium is the metallic base of the alkali, combined with hidrogen, the real alkali being a metallic oxide, which in common potash is combined with water. In its decomposition the oxigen both of the oxide and of the water is separated, while the metallic base and the hidrogen, or part of the hidrogen, combining, form potassium; and in the combustion of this product, its base and its hidrogen unite with the oxigen consumed, forming an alkali combined with water, and therefore not superior in alkaline strength, at least not to a great extent, to the potash in which water is combined.

The experiment made in a different mode appeared to give a different result.

Wishing to avoid every source of fallacy, I thought of a different mode of conducting the experiment, and this I have to state has afforded a result somewhat different. Mr. Davy had inferred from the combustion of potassium in oxigen gas, that 86·1 of it combine with 13·9 of oxigen to form 100 of potash. From its oxigenation by water, he had stated the proportions at 84 of base and 16 of oxigen. The former appeared to be the most direct mode, and I therefore preferred the estimate founded on it. Supposing then 86 of potassium to unite with 14 of oxigen to form 100 of potash, I took 4·3 of potassium, which according to this estimate are equivalent to 5 of real potash, and exposed it (the vessel being covered with a slip of muslin) to a humid atmosphere, until it was converted into a solution of potash. This I found required for neutralization, taking the mean of several experiments, of which the results varied very little, of the diluted acid employed in the above experiments 154 grains.

Here the power of neutralization in the alkali, formed by the oxigenation of potassium, supposing it to consist of 86 of base, and 14 of oxigen, is considerably superior to the power of neutralization of an equal weight of ignited potash, the one being 139, the other 154. It appears to have been by this mode of experiment, that chemists have determined the comparative alkaline strength of oxigenated potassium, and fused potash; at least I have seen no accounts of any attempts to do this by experiments on the real product

product of the combustion of potassium. Hence the agree ment in the conclusion, that the alkaline strength of th former is superior to that of the alkali in its real state.

Calculation of the quantity of acid, that should have been neutralized.

To perceive more clearly to what conclusion this result supposing it to be correctly established, leads, it may b calculated what quantity of acid the quantity of real alkal from 4·3 of potassium ought to neutralize. Five grains o ignited potash, containing 14 per cent of water, neutraliz 139 grains; five grains of real potash, free from water ought, according to this, to neutralize 162 grains; whic is the quantity of course which the 4·3 of potassium ough to have neutralized, on the supposition that in combining with oxigen it forms the real alkali.

Inferences.

Now although the alkali produced from the oxigenatio of potassium is thus inferior to real potash in neutralizin power, a result which so far supports the speculation have advanced, yet it appears to be superior considerabl to the ignited potash; and this may be supposed therefor to invalidate the first series of experiments, in which th alkali formed by the combustion of potassium was found little superior to ignited potash. To admit of this latte

Objections to these.

conclusion, however, it must be supposed, that th proportions in which potassium and oxigen combine to form pure potash are accurately estimated; and thi can scarcely be considered as determined with perfect certainty. No experiment is more exposed to sources o errour, both from the different degrees of oxidation of which potassium appears to be susceptible, and the action which it exerts during its combustion on the substance with which it is in contact. These become too, of much greater impor portance in the experiment, and even the accuracy of th estimate, independent of them, becomes more doubtful, from the smallness of the quantities which have hitherto been operated on. If from any of these circumstances th proportion of oxigen has been underrated, even to a smal extent, the difference between the result of the above expe riment, and of that in which the alkaline strength of the product of the combustion of potassium was directly ascer tained, will be accounted for.

Another

Another circumstance is to be attended to, of much importance with regard to the conclusion, so far as it relates to the general theory. The estimate of 14 per cent of water in ignited potash is the lowest that has been made. Berthollet himself, on whose authority it rests, has considered it probable, that there is a larger quantity; the abstraction of the whole, even by its decomposition, being prevented by the powerful affinity exerted to the last portions. By d'Arcet the proportion has been stated at 27 in 100 parts. Mr. Davy has inferred from an experiment, in which potassium was converted into muriate of potash in muriatic acid gas, that fused potash must contain nearly 23 per cent of water; and more lately he has remarked, that, if the proportion were to be judged of from the loss of weight in combining common potash with boracic acid, it would appear to be from 19 to 20 per cent. If we take the lowest of these numbers, 20, then the five grains of ignited potash, containing 20 per cent, neutralizing 139 grains, 5 grains of real potash ought to neutralize 174, and this is the quantity that ought to have been neutralized by 4·3 of potassium, supposing it to form the real alkali by oxigenation.

The calculation farther affected by the water in potash not being precisely determined.

The result, therefore, from this mode of experiment cannot be considered as invalidating the result from the more direct mode of trying the alkaline strength of the actual product of the combustion of potassium, and even by itself is not incompatible with the conclusion, that a portion of hidrogen exists in potassium, but rather serves to establish it.

The result, therefore reconcilable with the hypothesis.

The circumstance, that there is no sensible evolution of hidrogen in the decomposition of common potash by galvanism, still appears to me to go far towards establishing the opinion, that hidrogen enters into the composition of potassium. Mr. Davy, in reply to Mr. Dalton, who had stated this in support of the opinion of Gay-Lussac and Thenard, has lately remarked, that, in using a weak galvanic power, such as that which he employed in his first experiments, and by which small quantities only of the metals are procured, there is no sensible effervescence; but that, when from 500 to 1000 plates are used, there is a violent effervescence, and a production of hidrogen, and sometimes of potassuretted hirogen,

Argument from no evolution of hidrogen opposed by its evolution under certain circumstances.

But this accounted for by the decomposition of the water in the potash.

hidrogen, connected with the formation of the metal. This think, admits of a different conclusion from that which is i tended to be drawn from it. Water is certainly more easy decomposition by galvanism than potash is; when the g vanic power therefore is sufficiently strong to decompo the latter, it must be able to decompose the former; and course, when common potash, containing water, is deco posed by a galvanic battery, this water can scarcely be su posed to escape decomposition. And the hidrogen of su a portion of water cannot be supposed to pass off unc served, for although the hidrogen in water is to the oxyg only as 15 to 85 by weight, yet it is as 2 to 1 nearly l volume: and hence in the decomposition of water by galv nism, the disengagement of elastic fluid at the negative si is much more abundant than at the positive. It is farth to be considered, that, from the small quantity of oxyg which enters into the composition of potash, and the lar quantity of it which exists in the composition of water, t quantity of it belonging to the water, which common fus potash contains, supposing this to be only 15 per cent, is equ to the quantity which is contained in the real alkali. both, therefore, are decomposed in the galvanic actio half the quantity at least of the oxygen disengaged at t positive wire must be from the water, a quantity of hidr gen double the quantity of this, that is equal in volume t the whole oxigen evolved at the positive side, ough to appear at the negative side; if an effervescence, there fore, is perceived at the former from the disengagement c oxigen, an equal effervessence ought to be conspicuous a the latter; and, since it is not, we can scarcely avoid th conclusion, that the hidrogen, or at least much of it, ente into the composition of the globules of metallic matte which appear at the negative side. With regard to the fac that when the potash is under the action of a very powerfu battery, there is a violent effervescence, and production o hidrogen, and potassuretted hidrogen, I suppose this mus arise from the intense heat excited at the point of decompo sition, by which an elastic combination of potassium an hidrogen is established. If there is a certain degree of gal vanic energy, at which hidrogen, pure, or not holdin an

Farther arguments for the existence of hidrogen in the metalloid produced.

any potassium in solution, is expelled, it would deserve to be made the subject of experiment whether the metal obtained is precisely the same with the metal procured when there is no sensible disengagement of hidrogen; though still it is possible, that in all cases a portion of the hidrogen of the water in fused potash may be disengaged, which becomes only more evident when a large quantity of the alkalis is decomposed by a powerful galvanic battery. It can scarcely however be supposed, that the whole of the hidrogen is disengaged, else it must be apparent, if the above observations be just, even in the experiment where the weakest power is employed, for the volume of elastic fluid to be disengaged at the one side, if the water is decomposed (and of this decomposition scarce a doubt can be entertained) is as large as that disengaged at the other.

When potassium is procured by means of iron, the hidrogen may be expelled from that portion which combines with the iron.

In the method of procuring potassium by the action of iron on potash, at the temperature of ignition, a disengagement of hidrogen gas takes place during the process; a fact which may be supposed to prove at least, that the whole hidrogen of the water contained in fused potash does not enter into the composition of the potassium which that potash yields. This may, perhaps, be admitted; it is possible, as I have already remarked, that a portion only of the hidrogen exists in the potassium. Yet in drawing this conclusion one circumstance is to be attended to. The greater part of the base of the potash is obtained, not insulated, but in combination with iron. The hidrogen of this latter portion may be expelled, the iron combining with the real metallic matter, and this may afford the greater part of the hidrogen that is disengaged.

Compounds of potassium and hidrogen formed.

Potassium has an affinity to hidrogen. It combines with that gas so as to form an elastic compound; and, according to the experiments of Gay-Lussac and Thenard, it also combines at a high temperature with a large quantity of hidrogen, and forms a solid substance of a light gray colour, from which hidrogen is disengaged by the action of water. Mr. Davy was unable to obtain this result; but these chemists, aware of the objection he had made to their experiment, have more lately repeated it, and have affirmed, that, when potassium is heated in hidrogen gas, a quantity of the

gas

gas is absorbed, and a solid compound of potassium a hidrogen formed. These facts, as proving the existence an affinity between potassium and hidrogen, are favoural to the supposition, that it may consist of a base combin with a portion of hidrogen; especially as potassium never formed, where hidrogen is not present. Sodium scarcely visibly soluble in hidrogen; it appears therefore have a less strong affinity to it, and may be supposed to tain a smaller quantity in combination with it. And fr this may arise, in conformity to the view I have given my former paper, the greater specific gravity of sodiu and the inferior attraction of soda to water.

Sodium has less affinity for hidrogen.

The two lightest of the common metals combine with hidrogen.

The lightness of the metalloids is probably owing, I ha stated, to the combination of real metals, which form th bases, with hidrogen; the lightest of them, potassiu probably containing the largest portion. It is a curio fact in conformity to this view, that the two lightest meta tellurium and arsenic, are those which have the most pec liar relation to hidrogen, and evidently exert an affinity it. Ritter observed the singular fact, that, when telluriu is employed as the negative wire in decomposing water galvanism, hidrogen gas is not evolved, though oxigen given out at the positive side; at the negative a powder fa down, which he found reason to conclude is a compound tellurium and hidrogen*. The observation is an importa one, as proving a combination to be established by galvani between a metal and hidrogen, though in this case t quantity of hidrogen combined appears to be so large, to cause the metallic qualities to disappear; as happens t in the combination of potassium with a large proportion hidrogen in the experiment of Gay-Lussac and Thena above quoted †. I have found, that, in decomposing wa by galvanism, if arsenic be the metal immersed in the wa at the negative side, while platina is at the positive side, l

Arsenic combines with hidrogen by galvanic action.

* See Journal, vol. xxiv, p. 318.

† These facts with regard to the loss of metallic properties wl hidrogen in considerable proportion is combined with a metal unfavourable to the hypothesis, that it is a metallic body in aerial form. The properties of its oxide, water, are equally verse to this exposition.

Hidrogen not an aeriform metal.

hidro

hidrogen is evolved, than when platina forms the negative wire; and apparently less even than the quantity of gas discharged at the positive side. A portion of the hidrogen therefore must be retained in combination with the arsenic; and if arsenic acid slightly moistened be submitted to galvanic action in a platina cup connected with the positive side, on touching it with a platina wire from the negative side, dense white vapour arises, which is probably not from mere volatilization, but from the action of hidrogen on the arsenic. It deserves to be remarked too, that arsenic is of any of the metals the one most soluble in water in its oxidated state.

Its oxide most soluble in water.

All the facts I have stated in this paper appear to concur in giving probability to the view I have ventured to propose of the nature of potassium and the other metalloids—that they are metals combined with hidrogen.

I have the honour to be,

Yours, &c.

J. MURRAY.

VI.

On the Affinity of Muriatic Acid for different Bodies, considered with Regard to Mr. Davy's *Ideas of its Nature. In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

THE more I reflect upon Mr. Davy's new Theory of the nature of oximuriatic acid, the more I am convinced, that it is incomplete, and inadequate to explain the mode, as well as the nature of its combination with different bases. You were good enough to place in your Journal for last month some observations of mine relative to its inapplicability to some phenomena, which occur in the production of the hyperoximuriate of potash. Permit me now to call your attention to some facts relative to the affinity of muriatic acid for different bodies, which appear to me to be totally subversive of this ingenious hypothesis.

Remarks on the attraction of muriatic acid for different bodies.

Decomposition of muriate of soda by sulphuric acid.

Muriate of soda, according to Mr. Davy, is a compou of oximuriatic acid and metallic sodium. When sulphur acid is added to it, it is decomposed by complex affinit The water, which is contained in this acid, furnishes h drogen to the oximuriatic acid, which thus becomes muri tic; and oxigen to the sodium, which is then saturated l the sulphuric. This explanation appears at first sight pe haps satisfactory. But let us examine it nearer; water know, is composed of oxigen and hidrogen. The *propo tions* of each are *fixed* and *determined*. Sodium we lik wise know combines with oxigen, as upon the new hyp thesis does oximuriatic acid with hidrogen. But the pr portions in which these unite are also *fixed* and *limite*

Now is it not *primâ facie*, at least, a very extraordina circumstance, that the quantities of oxigen and hidroge which exist in water, should exactly correspond with t proportions in which sodium combines with the one and o muriatic acid with the other? And we must suppose t the case. The component parts of water, thus separate must either enter wholly into the new compounds forme or they will make their appearance each or either in its o peculiar form. In the instance before us neither of th appears in this form, therefore we must look for th in the new combinations. Now is it not a very uncomm coincidence, that there is no surplus in any of these ing dients? That the entire quantity of oximuriatic acid berated from the muriate of soda agrees so exactly with t hidrogen which is set free from the water, that the wh quantity of both form muriatic acid; while *at the sa time* the entire quantity of sodium likewise liberated tal as exactly with the remaining oxigen?

Decomposition of nitrate of silver by muriatic acid.

An inverse instance of the same improbability occu when muriatic acid is added to a solution of nitrate silver. Here a precipitate is formed, which, acco ing to Mr. Davy, must be oximuriatic acid and silv Hidrogen is disengaged from the muriatic acid, and oxi from the oxide of silver, which existed in the nitrate, quantity sufficient, neither superabundant, to form wate

But we need not argue on improbability,

But we are not thus left to argue upon *improbabilit* we have it in our power to show whether such recipr cha

change of parts does take place or not. We know the relative quantities of the ingredients in the compounds, which we originally submit to reciprocal action, and these quantities we must account for. But if, from established data, and collateral evidence, we know that the constituent parts of the original compounds cannot wholly combine in the actual proportions in which they are known to exist in the new forms, and if we have no means of accounting for any such surplus or deficiency, the hypothesis must fall to the ground. we may form calculations.

Let us thus endeavour, by the aid of long established and well authenticated analysis, to explain the synthetic operations, which take place in the instance above alluded to, viz. the decomposition of muriate of soda by sulphuric acid. Example in the decomposition of muriate of soda by sulphuric acid,

Water, which fulfills so important a function in these new arrangements, is composed of 85 per cent of oxigen, and 15 per cent of hidrogen, (in round numbers.) Muriate of soda consists of 59 parts basis, and 41 acid. Oximuriatic acid unites, according to Mr. Davy *, with its own bulk of hidrogen, to form muriatic acid. The relative weights of these must then be nearly 97 † parts of the former, and 3 of the latter. Sodium, upon the same authority, combines with 22 per cent of oxigen to form soda.

Now let us suppose, that 97 parts of oximuriatic acid are disengaged from their union with the sodium, by the action of the sulphuric acid ‡. These 97 parts will instantly seize upon three parts of the hidrogen of the water, and form muriatic acid. Thus there will remain 135 parts of sodium, and 17 parts of oxigen. These ought to combine to form soda, which the sulphuric acid would then saturate. But

* See Journal, vol. xxvii, p. 325.

† Gay-Lussac states oximuriatic acid to be heavier than common air, in the proportion of 247 to 100, which, taking the weight of 100 in that of the latter to be 31 grains, gives 76·5 for the weight of the former.

‡ The manner in which the sulphuric acid acts is by no means satisfactorily accounted for. No affinity can possibly subsist between it and *metallic* sodium; by what complicated agency then is the oximuriatic acid first disengaged?

upon the scale above laid down, here is a deficiency of no less than 21 parts of oxigen for the complete oxidation of this quantity of sodium.

If we suppose, that upon the action of the sulphuric acid the sodium first becomes saturated with oxigen, the difficulty will not be diminished. For in this case there would be an immense surplus of hidrogen, above what was necessary for the formation of muriatic acid.

and in that of nitrate of silver by muriatic acid.

Thus likewise, in the solution of silver in nitric acid, before noticed, the oxide contains 92 parts of metal, and 8 of oxigen. Twenty parts of muriatic acid are required to saturate these 92 parts, which only gives 0·6 of a part of hidrogen to combine with the 8 parts of oxigen. But by the foregoing proportions, 1·4 of hidrogen are required for the complete conversion of this quantity into water.

The proportions above laid down I have taken from the best authorities; and I believe that the calculations founded upon them are correct. But even supposing it possible to adjust either of the above examples to the exact relative quantities necessary to support Mr. Davy's hypothesis, yet instances of the like discrepancy are so numerous and so various in their particulars, that to attempt to reconcile them is a hopeless task.

Argument from the combination of muriatic acid with metallic oxides.

But there is another objection, which is more simple and more self-evident, than that above stated. In the case where the oxides of metals give up their oxigen to combine with the hidrogen of muriatic acid to form water, as the quantity of hidrogen in muriatic acid must be definite and limited, the quantity of this acid required to saturate the different oxides ought to be in *direct* and *exact* proportion to the quantity of oxigen which they contain. But this is not the case. The oxide of potassium contains 13 per cent of oxigen, and combines with 36 per cent of muriatic acid. The oxide of sodium contains 22 per cent oxigen, and ought, therefore, in this proportion to unite with 60 per cent at least of acid. But 41 per cent will be found to be the utmost limits of its union.

It is useless to multiply examples. One such objection if founded in fact, would be sufficient at once to overturn the longest established, and, in other respects, the com-

pletes

pletest theory. I will, therefore, trespass no farther on the limited space of your valuable Journal. By giving a place in it to the above observations, you will render an important service to the cause of chemistry; as they must either draw from the ingenuity of Mr. Davy a reconciliation of the differences, and thus confirm, by undeniable and irresistible evidence, the justness of his ideas; or they must inevitably confirm the insufficiency of his ingenious hypothesis. For I am certain, that no one can for an instant doubt the justness of the principle upon which they are founded, viz. that in the case where two compound bodies are submitted to mutual decomposition, the elements of these bodies must either be totally absorbed, and (if I may be allowed the term) neutralized by a fresh combination; or they must appear in their own peculiar form, and with their own distinguishing characters.

I remain, Sir,

Your most obedient servant, and constant reader,

13*th of April*, 1811. F. D.

VII.

Description of an economical Process for Evaporation, invented by the late Mr. Joseph Montgolfier: *by Messrs.* Desormes *and* Clement *.

Spontaneous evaporation applied to economical purposes.

CONSIDERING the great effect of spontaneous evaporation, that takes place from the simple contact of air with humid substances, or with water itself, Mr. Montgolfier imagined, that the same means might be employed for drying without the assistance of fire a great number of matters, which are liable to be injured by its application. The instances of natural salt-pits, and graduation houses, occurred to him, in which evaporation is conducted very economically: but the former are adapted only to hot climates, and the latter depend too much on the state of the atmosphere, to be applicable to the principal object he had in view, the desiccation of alimentary substances; an operation of which the object was to preserve them a long time

* Abridged from the Annales de Chimie, vol. lxxvi, p. 34.

without

without alteration, and without the addition of sugar, sal or other matters, which are expensive, and alter their na tural flavour.

Concentration of must for making wine in northern countries;

He had formed, in particular, the project of concen trating the must of grapes previous to its fermentation, i order to convey it at small expense to northern countries there to make it into wine by the addition of a suitabl quantity of water, and then fermenting it. The idea wa grand, and perhaps at some future period will be realized but in the mean time the fabrication of grape-sugar is a important object for such a process.

This process requiring but a very moderate temperature which may be that of the atmosphere alone, will render th sirup of grapes exempt from the empyreumatic flavou commonly found in it, though it does not exist in the fruit and probably occasion its use to become more extensive thus contributing to the accomplishment of the benevolen views of government.

and of the juices of fruits. Improvement in sugar.

It is equally applicable to the evaporation of all th juices of fruits, and would be particularly advantageous t that of the sugar cane, as it would render the raw sugar much whiter, and more pure than those that usually come t us so loaded with an uncrystallizable melasses, which give them a bad taste.

Other applications of it.

The economy of the process too allows its application t every process of evaporation, that does not require a hig temperature. It might be employed in the salt-works in th east of France, in saltpetre manufactories, &c.

Delicious conserves in large quantity.

Montgolfier's first trials were made in 1794. He prepare conserves of several fruits, and among others of apples an grapes. The former, which was in large quantity, for h made above three thousand pounds weight, had so fine flavour, that the fruit itself appeared disagreeable in com parison to it. He repeated his experiments at Paris, i 1797, with results equally satisfactory, except that the pro ducts were not so good as those afforded by the fruits o Dauphiny; indeed there seemed to be more difference betwee the conserves than between the fruits of the two countries.

The publication of the hydraulic ram *, and its improve ments, turned his attention from the present subject, which

* See Journal, vol. xiv, p. 98.

though

though important, was less interesting, and he thought no more on the subject. The results, however, are too valuable, particularly at the present moment, to be kept longer secret.

Principle of the process.

As the air has the property of evaporating water at all temperatures, we have only to bring a large quantity into contact with the wet matter we would dry, or the liquid we would evaporate, to obtain a great effect in a little time.

If we would obtain this effect in a small space, which is the object of almost all manufacturers, we must cause a great deal of air to pass through this space in a little time by producing a rapid current. A very simple machine, not expensive, and well adapted to this purpose, is known by the name of a centrifugal ventilator*.

Advantages.

Thus in evaporation, without artificial heat, the caloric necessary for the production of vapour will be that of the atmospheric air: but it will be the free caloric, and not that of combination, which combustibles alone can disengage. To produce the union of the free caloric of the atmosphere with water, simple contact alone is required; and this contact may be effected by power. Power, therefore, is the only expense required, to obtain this combination of water with caloric. It will be shown presently, that this is much cheaper than fuel: besides, the former is always at our disposal, while the latter becomes every day more expensive, in consequence of the consumption of wood and of charcoal. We have therefore a real advantage, general and particular, in the process of evaporating without artificial heat.

Heated air may be employed,

Sometimes the atmospheric air is so damp, that it would dissolve but little water, and its being set in motion would profit but little. Such cases, however, are very rare; and we see every day, even during rain, that the wind is drying. But to hasten evaporation, and regulate the process, the air may be heated before it enters the evaporating machine: it might be passed through a fire of charcoal, if the subject under evaporation would be injured by smoke; or of any

* It is described by Desaguliers in the Philosophical Transactions for 1735; but he does not appear to have assigned the best possible dimensions to the machine.

combustible

combustible in other cases*. We should thus obtain the same advantage as by employing the atmospheric air at its proper temperature. With the air heated by the fire cold air might be mixed, so as to make the temperature of the mixture 30°, 40°, or 50° [86°, 104°, or 122° F.] at most. Beyond this we are not certain, that the juices of fruits would not be altered: at least Montgolfier stopped at this point in his experiments.

with a saving of fuel.

Though in this case fuel is employed to transmit to the water the latent heat of the air, there is still a saving of expense. Here the caloric evolved by combustion is well mixed with the air, which carries off almost the whole of it, and easily parts with it to the water, from the vast surface this presents in the apparatus to be described. Besides if the air contained a little free caloric before the combustion, it would still present it to the water as in the evaporation without artificial heat; and if it escape at a lower temperature, than it had when it entered, we may be sure we have employed the fuel to good purpose. In the ordinary mode of evaporation we are far from enjoying these advantages: the caloric is obliged to traverse substances of considerable thickness and little surface, to combine with the water; and the air escapes rapidly from its contact with the surfaces, and carries with it a considerable quantity of heat.

Calculations of the effect.

We have here no new idea, but the result of well known truths; the advantages of which have been overlooked. To form a proper judgment of it, we should know with Montgolfier, that, in the common state of the air in autumn, a cubic foot of air, by its contact with water, will evaporate from one to four grains, or at a medium $2\frac{1}{2}$ grs. We will suppose it only 2 grains, the proportion adopted by Montgolfier from his experiments in Dauphiny. To put the air in motion, like any other body, requires more power for a greater velocity; and this power is proportional to the square of the velocity. That is, if a man by a day's labour, as is the fact, can give a velocity of $7\frac{1}{2}$ feet [8 feet Eng.] per second to 8100000 cubic feet [9800000 Eng.]

* The air to be heated should not be passed through the naked fire, but through a tube or tubes heated by the fire. C.

it

it would require as much power to give twice the velocity to one fourth the quantity of air.

This velocity, or that of 16 feet per second to 2450000 cubic feet of air, is well adapted to the present case, as it does not require too great a power, and the dimensions of the apparatus will not be inconvenient. We will suppose a man to work at this rate for an hour, then to rest as long, and after six hours of actual labour to have accomplished his day's work. Having thus passed over the evaporating matter 2450000 cubic feet of air, he will have produced an evaporation of 4900000 grs, or rather more than 100 wine gallons of water; and, as the pay of a day-labourer at Paris is only 1 f. 50 cent. [15d.], such a quantity might be evaporated at this expense. To evaporate a similar quantity by fire would take about two bushels of coals, which at Paris would cost from 2s. 6d. to 3s. The labour of a horse would be rather more expensive than that of a man, and would require a larger apparatus to be applied to the best advantage: but where a larger machine is desirable, a greater moving power is requisite.

Having explained the principles of the process, we shall proceed to describe the apparatus; observing, that not merely the dimensions, but the arrangement of several of its parts may be considerably varied, without detriment to the effect.

Apparatus described.

Pl. X, fig. 1, is a perpendicular section of the machine; and fig. 2 is a plan of it in the line A′ B′.

A B is a handle 15½ inches long, making a circuit of 49 inches, which a man turns round in a second.

B C is an axis, having at its extremity C a toothed wheel, taking in a cogwheel D, which has twice as many cogs as the wheel has teeth, so that it makes but one turn while the wheel makes two.

The cogwheel D is fixed on an axis D E, which rests by a pivot on the support E, and is kept vertical at F by a brass collar, well fitted to it, but admitting it to turn with ease.

The axis D E carries six or eight wings 4 feet 10 inches long. The ribs of these are of iron, and they are covered with oilcloth, or varnished cloth: for they should be both strong and light.

This

Apparatus described.

This mill turns between two large circular planes, wh extend a little beyond the ends of the wings. These pla form the top and bottom of a kind of drum, the circu ference of which is open, except two hoops, one at t the other at bottom, about 6 inches broad; and conseque ly leaving between them an opening of 371 inches by 7·8 or 2916 square inches; for I suppose the distance betwe the top and bottom of the drum to be 19½ inches.

The wings of the ventilator should be as broad as possib so that they can move without friction.

The top and bottom of the drum are connected of cou by pieces of wood at their circumference, which will minish the opening. This may accordingly be estimated 2400 square inches only: but this is of little importan as it is sufficient if it be equal to the inhaling orifice, whi we shall estimate at 1000 square inches.

The bottom of the drum has a circular hole, G H, least 36 inches in diameter. The free surface of this sho be 1000 square inches.

To this is fitted a cylindrical tube, I K, of the same diam ter, and descending perpendicularly about 40 inches. is supported so as not to rest on the lower part of t apparatus.

L M is a lid for the square box N O P Q. It is perf rated with a hole corresponding to the tube I K, and fro this hole rises the end of a cylinder similar to that abo which fits exactly. Paper is to be pasted over this joi that no air may escape.

The box N O P Q is a rectangular prism, 8 feet 2 inch square, by 5 feet 9 inches high, in which are placed twigs birch, without bark, and very clean, in regular beds, crossi each other alternately, and leaving more open space at b tom than at top. On this heap is dispersed the liquid to evaporated, which enters by small holes in the lid, so as be distributed as equally as possible amongst the twigs.

Between the twigs and the lid a space should be left f in the middle at least a foot high, not to check the passa of the air. The twigs also should not be too close, b placed so that no horizontal section should contain less th 3000 square inches nearly.

T

The bottom of the box is a wooden grating, the bars of which are very open; and about three feet beneath it is a vessel to receive the concentrated fluid. There it will receive the first impression of the open air, and be still more concentrated. Apparatus described.

In the case we have noticed, when the air is not sufficiently drying, it may be heated by passing it through a furnace, the chimney of which widens to at least 1000 square inches, and takes in fresh air by sufficient openings for the purpose. This pipe should be divided into four others of 250 square inches each, communicating with the openings at the bottom of the four sides of the box.

After the description here given, it is obvious, that the ventilator, turned by the man, throws the air from between its wings to the circumference, and draws in fresh air at its centre, which air traverses the whole heap of twigs, and consequently the fluid. If this be not sufficiently concentrated in one descent, it may be pumped up again, and suffered to fall as often as is necessary.

Montgolfier attempted to do without the twigs, by forming small jets of fluid in the box, the height of which he increased: but this mode is not economical with regard to expense of power. They might be dispensed with, however, by adopting the following contrivance. In the lower vessel, that receives the fluid, place a wooden roller, turning on its axis. At the top of the box, which in this case should be long and narrow instead of square, place a similar roller, with a winch affixed to it without. Over these two rollers pass an endless band of cloth, and this, on turning the winch, will raise the fluid, and expose it to the air on an extensive surface.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT the meeting of this society, on the 16th of February, Professor Jameson read a minerealogical description of the romantic and interesting valley of Pentland, near Edinburgh. Valley of Pentland, near Edinburgh.

burgh. After describing the magnitude and direction of t valley, and the shape and height of the mountains s rounding it, the Professor stated as a probable conjectu that it was formed by a lake, which had emptied itself b lateral valley that traverses its south side. He next e merated and described the various rocks of which it is co posed, and gave a full detail of their geognostic relatio Four sets of rocks were described as occurring in this v ley; transition rocks, as sandstone and conglomer rocks,—rocks of trap-tuff,—wacke,—amygdaloid, &c and rocks of the clay-stone series, as clay-stone, porph roid, compact-feldspar, &c.

Other communications.

At the meeting on the 9th of March, the Secretary re communications from Dr. Thomas Thomson, containing analysis of iron-ore from Greenland; from Dr. Edmon ston, Shetland, on the larus glaucus; and from Dr. Barcla on the structure of the cells of bees and wasps.

Culture of Jerusalem artichokes.

The Free Society of Arts of the Department of the Sarth has offered prizes for those who shall have cultivated wi most success Jerusalem artichokes, and mangel wurzel, n less than an acre of each. It is observed that the Jerusal artichoke is particularly advantageous as winter food f Merino sheep, which are exceedingly fond of them. Th are best given in the proportion of two pounds with one dry fodder. A field, that would produce but 1500 ratio of lucerne, will yield 5000 rations of Jerusalem artichok The stalks are good fuel, or when green may be given cattle. Cows, hogs, and horses, eat the root as well sheep. The plant is liable to no injury from drought summer, or frost in winter; and no insect or disease h been found to check its vegetation.

Prize for flax-spinning machine.

In May last the Emperor Napoleon published a decr dated from Bois-le-Duc, in which he offers a prize of o million of francs [£41666] to the inventor of the best m chine for spinning flax. The prize is offered without a restriction in regard to time, or to the country of t inventor.

Grape sugar.

Mr. Proust having extracted from the sirup of grapes concrete sugar; and Mr. Fouques having found means whitening it, and giving it the colour and consisten

thou

though not the sparkling appearance [*le brillant*] of sugar from the cane; a reward of 100000 francs, [£4166] has been bestowed on the former, and 40000 francs, [£1666] on the latter. The sweetness of this sugar, however, is much inferior to that from the cane, as in this respect one pound of the latter is equivalent to at least 2¼ of grape sugar.

Congreve's rockets.

At a meeting of the Italian Academy, in January, 1810, Mr. Hess, of Zurich, read an account of a method of preventing the effects of Congreve's rockets.

Tin mines discovered in France.

In 1795, wolfram, which commonly accompanies veins of tin, was discovered in France, near St. Leonard, in the department of the Upper Vienne, and some search was in consequence made for tin, but without success. Lately, however, farther search has been made by Mr. de Cressac, mine engineer, and he has found tin in the state of oxide, and characterised by its peculiar crystallization. Mr. Descotils has analysed it, and obtained from it very pure tin. The vein was found to be more rich, in proportion as it was traced to a greater depth, and it is expected may be worked with advantage.

New land is covered.

Mr. Hedenstroem, in the Russian service, discovered in the summer of 1809 a land in the Frozen Ocean, to which he gave the name of New Siberia. The part he explored he has called the coast of St. Nicholas. His observations will be found interesting to the Natural Historian, as well as to the Geographer.

Fresco painting transferred to canvas.

Palmaroli has succeeded in removing the Interment of Christ, by Daniel de Volterra, from the wall of the church of the Trinity on the Mount at Rome, on which it was painted in fresco; and transferring it to canvas, to be conveyed to Paris.

Substitute for tobacco.

The dried leaves of the cherry-tree are now employed by the Norwegians as a substitute for tobacco.

Preparations of phosphorus.

Mr. Curaudau procures phosphorus by mixing one hundred parts of calcined bones, thirty of potash, twenty of sulphur, and fifteen of vegetable charcoal, and distilling the mixture in a stone retort by a strong heat. The phosphorus that comes over commonly contains a little sulphur, but it is not the less fit for the fabrication of

of phosphoric matches, which require a mixture of substance.

Improvement of the tone of wind instruments.

A musician of Avallon, in France, professes to discovered a method of giving to all kinds of wind in ments made of wood that improved toue, which they g rally acquire only after having been used for a conside time, and which is afterward liable to be impaired. It parts to a new instrument all the perfection of one tha been ten years in use, and renders it permanent.

Soles of shoes fastened by screws.

A Frenchman, who professes to make screws superi quality to the best of English manufacture, recomm them strongly for fastening the soles of boots and sl He says they occasion a saving of three fourths, fron great durability of the shoes thus made; and that the not make a noise like nails, as their heads imbed thems in the leather.

Treatise of mathematics.

A complete course of pure Mathematics, for the us the pupils of the Normal and Polytechnic Schools, has published at Paris, by Professor L. B. Francœur, in volumes. Its method is much commended.

Meteorological Journal.

Professor Vassalli Eandi commenced, in 1809, a meteo logical Journal, under the title of Annals of the Obse tory of the Academy of Turin, with statistical notes r tive to Agriculture and Physic. It appears to embrace m information from its plan, which is as follows: 1. The of the month, the moon's age, daily declination, pha apogee, and perigee; height of the barometer, and of thermometers, one facing the south, the other the north sun-rise, noon, and sun-set. 2. Observations on the moscope, anemometer, hygrometer, hydrometer, and a dometer, at the same hours. 3. State of the atmospher the same periods. 4. Summary of the meteorological servations for each month, with the extraordinary ph mena, if any occur. 5. Comparison of these observat with meteorological sayings, both general and relatin the state of the moon. 6. Natural epochs, namely, appearance of birds of passage and insects, and rural ob vations relative to the operations of agriculture, the flo ing of plants, the harvesting of different crops, the deva tions of insects, and the congress and births of dom anim

animals. 7. A Comparison of these with agricultural sayings, and the physical grounds of these sayings. 8. Marriages and births. 9. Diseases and deaths. 10. Diseases and deaths of domestic animals. 11. Price currents of different articles, with the reasons of variations in their price. To these it is the editor's intention to add, 1. Observations of the electrometer, magnetic needle, diaphanometer, cyanometer, photometer, and eudiometer. 2. A comparison of the meteorological aphorisms of Toaldo, Lamarck, and others, with the observations made. 3. Proceedings of the most skilful and active farmers for every month in mountainous districts, and in plains. 4. Bee journal. 5. Remarks on the daily bill of mortality, and the age and condition of the deceased, particularly with regard to sudden deaths, and the character of epidemic or dangerous diseases. 6. Observations similar to those made at Turin, carefully registered in the other departments of the 27th military division. 7. A comparison of these with similar observations in the departments of Montblanc and the Maritime Alps, in the 28th military division. 8. Various other observations, calculated to enlarge the boundaries of science. 9. Astronomical observations, as soon as the observatory is furnished with the requisite instruments.

Entomology of Switzerland.

Mr. de Clairville has published a second volume of his Entomologie Helvétique, the first of which appeared in 1798.

Memoirs of the Royal Academy of Munich.

The Memoirs of the Royal Academy of Sciences of Munich for 1808 contain several papers on botany, and some on comparative anatomy. Mr. Petzl has found the Bolognian spar in some marle pits near Amberg. He has also described a calcareous carbonate, lying under a thin coat of vegetable mould near Erding, in Bavaria, and there called *alm*, or *alben*, which destroys the fertility of the soil for some years, if it be turned up by too deep ploughing. According to him it is a calcareous tufa in a state of efflorescence, and should rank between chalk and bergmehl. We should suspect, from the experiments of Mr. Tennant on our limestones, that it contains magnesia. Some observations on direction and inclination of the strata in the primitive mountains of Norway and Sweden, by Mr. J. F. L. Haussmann, tend to confirm the theory of Mr.

Mr. von Humboldt. Dr. C. F. Bucholz, having attemp to analyse a sulphate of zinc containing copper in mode proposed by Mr. Vauquelin, has proved its insu ciency. He immersed a slip of zinc in a solution of sulphate, and found, that the zinc, instead of precipi ting metallic copper, acquired a blackish coating, whi when separated, dried with blotting paper, and rubbed w an agate burnisher, had the colour and brightness of br

Defence of fortified Places.

A treatise on the Defense of fortified Places, compo by order of the Emperor, for the Instruction of the Pu of the Corps of Engineers, by Mr. Carnot, is a work considerable importance in the military art. In an pendix he strongly recommends to the besieged, to howitzers loaded with grapeshot, or musket balls, at elevation of 45°, when the enemy have made their proaches within a certain distance. They may thus from behind the ramparts, without being exposed; their shot will do more execution, not being stopped the enemy's works. This is now made a part of French artillery exercise.

New Encyclopedia.

In the beginning of last year was announced at Pari prospectus of an "Instructive Collection of all the theo tical and practical Truths hitherto discovered in Natu History, Physics, Chemistry, Medicine, Surgery, Ag culture, Arts and Trades, and Domestic Economy, s jected to fresh Examinations, verified by new Observatio and confirmed by new Experiments." The work is to illustrated by upward of a hundred and fifty thous plates, many of them coloured, representing accurately the mineral substances that can be delineated, all kno plants and animals, all the machines, inventions, inst ments, and tools hitherto contrived, in France or otl countries, with portraits of persons who have distinguis themselves in the arts or sciences. It is to be the joint wo of a great number of literary men and artists of the vari parts of Europe; and is to be composed in four separ languages, French, English, Italian, and German, by n methods, the object of which is to render its executi equally prompt and perfect, and to enable persons of smallest fortunes to procure this useful collection a trifling expense.

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